THE

MEDICAL EXAMINER.

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Papers for publication must be received *before* the 16th of the month, or they cannot appear in the forthcoming number.

The following Journals have been received in exchange:

Medical News and Library, November.
New York Journal of Medicine, ditto.
New Jersey Medical Reporter, ditto.
American Medical Monthly, ditto.
American Medical Gazette, ditto.
New York Medical Times, ditto.
Boston Medical and Surgical Journal, (weekly.)
Buffalo Medical Journal, November.
Virginia Medical and Surgical Journal, ditto.
Stethoscope, ditto.
New Hampshire Journal of Medicine, ditto.
Ohio Medical Counsellor, (weekly.)
Memphis Medical Recorder, November.
Montreal Medical Chronicle, ditto.
Peninsular Journal of Medicine, ditto.
New Orleans Medical and Surgical Journal, ditto.
New Orleans Medical News and Hospital Gazette, ditto.
North-Western Medical and Surgical Journal, October.
Charleston Medical Journal, November.
Western Lancet, Oct.
Ohio Medical and Surgical Journal, November.
Western Journal of Medicine and Surgery, September.
Atlanta Medical and Surgical Journal, November.
Southern Medical and Surgical Journal, ditto.
American Journal of Pharmacy, ditto.
American Journal of Dental Science, October.
British and Foreign Medico-Chirurgical Review, ditto.
Dublin Quarterly Review, ditto.
Journal of Psychological Medicine, ditto.
London Lancet. (Weekly.)
London Medical Times and Gazette, ditto.
Dublin Medical Press, ditto.
El Porvenir Medico.
Revue Medico-Chirurgicale, to September.

BOOKS AND PAMPHLETS RECEIVED.

Wharton and Stillé's Medical Jurisprudence. Kay & Brother.
Lehmann's Physiological Chemistry. Blanchard & Lea.
Renouard's History of Medicine, translated by Comegys. Cincinnati.
Sargent's Minor Surgery. Blanchard & Lea.
Carson's Synopsis of Course. Blanchard & Lea.
Huss on Typhus and Typhoid Fever. London.
Transactions of the American Medical Association. Vol. VIII.
Jackson's Letters to a Young Physician. Boston.
Isaac's Anatomical Remembrancer. S. S. & Wm. Wood.
Parrish's Practical Pharmacy. Blanchard & Lea.
Epidemic Cholera, by J. H. Vanderveer, M.D.
Annual Report of the City Inspector of New York.
Discovery of the Cause, &c., of Epidemic Cholera, by Dr. Knapp.
Introductory Lectures by Profs. Dalton, Smith and Pope.

The foreign correspondents of the Examiner will please direct their Exchanges, Books for review, and other communications, to the care of Trubner & Co., No. 12 Paternoster Row, London, or Mr. H. Bosange, 21 Bis, Quai Voltaire, Paris.

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THE

MEDICAL EXAMINER.

NEW SERIES.—NO. CXXXII.—DECEMBER, 1855.

ORIGINAL COMMUNICATIONS.

Case of Poisoning with Arsenic, attended with remarkable delay in the appearance of the symptoms. By EDWARD HARTSHORNE, M. D.

The following brief sketch of a case of self-inflicted poisoning terminating favorably, is worthy of note, on account of the very unusually long interval that elapsed between the swallowing of at least one drachm, if not two, of arsenious acid, and the appearance of the first decided symptoms of its poisonous action.

I am indebted to the attending physician Dr. Hershey, and to Dr. Griffith, also in attendance after the first day, as well as to the patient herself and to the inmates of the room adjoining hers, for the account of the first four days of her case.

From this it appears that she was a strong, healthy woman, about twenty-two years of age, of a rather excitable nervous temperament, and headstrong impulsive disposition; that she had been subjected for some time previous to great mental agitation, through the threatened withdrawal of an acknowledged admirer, and that under this excitement she had suffered occasional attacks of hysterical spasms. According to her own representation, she had eaten and drunk very little throughout the

week ; and on Thursday, the twenty-ninth of March, 1855, she took no food or drink except one cup of coffee at breakfast time. She spent the day in a state of desperation, most of the time on foot, first in seeking an interview with the delinquent lover, and then in procuring from an apothecary, at a long distance from her dwelling place, a paper of arsenic. Her absence from home all day, and her entire abstinence while at home, were testified to by the other inmates of the house. At nine o'clock in the evening of the same day, she had retired to her room, and was heard by the two occupants of the adjoining chamber to be gagging and choking so violently and in such a peculiar manner, that one of them knocked at her door and inquired if there was anything the matter with her. She returned an evasive answer, and remained apparently quiet throughout the night. She kept in bed the next morning, and refused her breakfast, but attracted no special attention until nine o'clock, when the same sound of gagging and choking was heard in her room a second time. In the course of the succeeding two hours an hysterical paroxysm came on. It was then ascertained that she had taken poison, and Dr. Hershey was immediately sent for.

He arrived at 11 o'clock, A. M., just fourteen hours after the first dose had been taken, and two hours after the second. At this stage of the proceedings nothing definite could be learned from her admissions or complaints. She lay in a state of partial cataleptic stupor, occasionally varied with slight muscular spasms such as are common in ordinary hysteria, and seemed to be unwilling or unable to answer questions. The customary effects of irritant poison were so entirely absent, that Dr. Hershey was induced to order an anti-spasmodic potion of some aromatic tincture in camphor water, on the supposition that the attack was a purely nervous one alone. Of this draught she took some four tablespoonfuls, at intervals, in the course of two hours. According to her own repeated assertion, which there is no reason to disbelieve, it was the first fluid which she took into her stomach during at least thirty-six hours—that is for twenty-four hours before and some twelve or thirteen hours after the taking of the evening portion of the arsenic. No change beyond the abatement of the hysterical symptoms occurred until one o'clock in the afternoon, about two hours after

she had swallowed the first of the camphor water, when the doctor was hurriedly recalled on account of the sudden onset of violent pain and vomiting. The most frequent, and in this instance, at that time, the only positive, symptoms of arsenical poisoning had at last presented themselves, sixteen hours after the first powder had been swallowed, and four hours after the second.

The fresh hydrated oxide of iron was immediately given and continued, in divided doses, to the amount of five ounces; but notwithstanding this and the free use of sulphate of morphia and cold mucilaginous drinks internally, and of depletion with cups and the subsequent application of cataplasms and a blister externally, the pain and vomiting increased in severity and frequency until the afternoon of Sunday, the third day. She then appeared to be so utterly prostrated that no hopes were entertained of her recovery either by herself or her physicians. A paper containing a portion of the white arsenic of the shops had already been found in her room. The apothecary had been visited, and had confirmed her story as to its purchase on the preceding Thursday. The amount remaining in the packet had been compared with the quantity stated by her to have been swallowed. Questions were again asked her, when she was supposed by all to be in extremis, and were answered as frankly and fully as could be desired. These answers being given under the impression that she was at the point of death, are fairly entitled to belief for that reason alone; but they were corroborated wherever possible by other testimony, and invariably supported by her own statements in answer to the repeated and varied cross-questionings to which she was subjected by her medical attendants during convalescence, and after every inducement for a perversion or suppression of the truth had been removed.

The history thus gathered from the patient and her friends, was, in a few words, that her whole mind had become absorbed in the thought of her troubles, and the desire to end them in the grave; that with this intent, having secretly provided herself with arsenic, and retired to her room after a whole day of fasting and agitation, she attempted to swallow a teaspoonful of the dry powder and was so irritated in the throat by it as to alarm her neighbors. She coughs out a part of it, but manages to retain about half-a-teaspoonful. She lies down, as she

expects, to die, but spends the night without any change whatever, and, as she thinks, without sleep. The next morning she swallows another half-teaspoonful in the same manner and with the same difficulty. Soon after this last attempt, she loses her self-control, becomes hysterical, and announces her probable fate to the family who come to her aid. During all this time, however, she feels no nausea, thirst or pain, and has not the slightest idea of the agony she is shortly to endure. It is not until after she has taken several drinks, that she begins to realize the horrors of her situation. Her entire ignorance of the effects of arsenic was fully demonstrated, and of itself afforded satisfactory proof that, although intelligent and tolerably well educated, as well as a determined girl, she could not possibly have simulated her disease, or so contrived to regulate the dose as to effect a partial poisoning only. The symptoms, however, were too well marked in their course and violence to admit of any doubt as to the existence and progress of an irritant poison. Nothing more was required to demonstrate the presence of arsenic, except the chemical examination of the matter thrown up from the stomach, and of the fæces and urine. Unfortunately, this examination was not made. My attention was first called to the case on the evening of the third day, Sunday. The vomiting and pain had then ceased. Reaction had begun, although the patient was still in a partial collapse, with feeble, quick pulse, pale sunken countenance, cool, moist skin, great muscular debility, and a tendency to hebetude with occasional temporary cataleptic spasms. The pharyngitis, tympanitis and epigastric and hypogastric tenderness were very well marked. These gradually subsided, and were accompanied and succeeded by similar evidences of inflammation running along the course of the colon down to the rectum, and going off with tormina, tenesmus, bloody stools, hemorrhoidal irritation and strangury, and followed for a few days by an acne-like eruption of the skin.

The treatment, which was demulcent, anodyne and cautiously supporting, with a gradually improving regimen, need not be particularly detailed. The youth and general health of our patient secured her a comparatively rapid convalescence. By the end of five or six weeks, she had recovered her former flesh and

color, and appeared to have escaped with unimpaired gastric and intestinal functions. The only difficulty that remained, was a debility of the lower extremities, amounting to a partial paraplegia, and a relaxed condition of the pelvic organs, which led, on two different occasions, to a painful retroversion of the uterus. Both of these infirmities, however, were so far removed in the course of another month, that, at the latter part of June —within three months after her attempt—she considered herself perfectly restored to health.

It was of course impossible to determine the amount of arsenic actually swallowed in this instance. A sufficiently near estimate, however, was reached through the patient's own statement, made under circumstances that afforded no inducement for deception, and confirmed to a considerable extent by her fellow-lodgers, and by the apothecary who furnished the packet from which the two portions must have been taken.

Supposing her to have retained no more than three quarters of a teaspoonful altogether, which is the smallest probable amount, she must, according to the approximative measurements given by Taylor, have been exposed to the destructive action of at least one hundred and twelve grains in all, and to that of fifty-six of these grains, thirteen or fourteen hours before the appearance of any nervous symptoms, and just sixteen hours before the accession of pain, vomiting and thirst.

The cerebro-spinal demonstrations presented nothing sufficiently different from those of ordinary hysteria to warrant their being regarded as the narcotism sometimes produced by arsenical intoxication. Still, allowing these disturbances to have been the premonitory signs of more serious poisonous action, we yet have an interval of fully thirteen hours' duration, after the ingestion of nearly a drachm of the acid without the advent of a single irregular sensation.

Cases are reported in which arsenic has begun its work at once upon its victims. More frequently, a few minutes have elapsed; but the average period at which the deleterious operation generally begins to manifest itself, is stated to be from half an hour to an hour. Taylor refers to a case communicated to him by Mr. Todd, in which no symptoms appeared for two hours, after one drachm had been taken on an empty stomach. Orfila relates one

in which five hours passed, after more than half an ounce had been taken, without decided mischievous effect; Devergie reports an interval of four hours; Taylor quotes another of nine hours duration, and one from Belloc, (see Taylor On Poisons, p. 259,) in which the symptoms did not present themselves for ten hours. This last is the longest instance of uncomplicated freedom from noxious symptoms hitherto on record. There are others besides these, of different periods of protraction, which need not be detailed. The case of Tonnelier, (see Orfila, Toxicologie, 1, 386,) although very remarkable, is not entirely free from doubt, as an instance of retarded progress in arsenical poisoning. That of Clegg, also, (see Taylor, Med. Jurisp. 76,) can hardly be included in the class, because it must have been affected by the modifying influence of opium upon the arsenical irritation. I have not been able to find, in the Journals, any analogous cases not already cited in the works on medical jurisprudence; and I am, therefore, inclined to believe that our patient stands alone in her experience, so far at least as the published annals of toxicology have gone.

The bearing of such experience on the medical evidence in certain criminal cases, may be easily understood. Christison expressly recognizes the importance of it in his work on Poisons, (p. 237,) and refers to a trial, in which a woman escaped conviction, principally because "the symptoms of poisoning did not begin until more than eight hours after, the only occasion on which the prisoner was proved to have administered anything in a suspicious manner." "As I was not at that time," continued he, "acquainted with any parallel case, except that recorded by Orfila, I hesitated to ascribe the symptoms to the draught; and, consequently, as the other medical witnesses felt the same hesitation on the same account, the proof of administration was considered to have failed. I am not sure that I should have now felt the same difficulty."

Our case seems to me to possess considerable interest, also, in a physiological and pathological point of view; but as my object is especially to report the observation for what it may be worth in questions of medical evidence, I must leave to the ingenuity of other inquirers, all speculations as to the cause and nature of the delay of gastric disorder under the peculiar circumstances stated.

GLYCOGENIA.—THE SECRETION OF SUGAR IN THE HUMAN ECONOMY.

By Bernard Henry, M. D.

The present would seem to be an opportune moment for the examination of the question of the production of sugar in the system, and its physiological and pathological import.

It is now more than six years since Mr. Claude Bernard announced to physiologists, that sugar is a normal secretion in man and animals, and maintained by experimental proof that this function has its seat in the liver. The disease, diabetes mellitus, in which grape sugar appears in the urine, had been studied and observed with much attention ever since the time of Willis, in 1674, but its pathology was still obscure, and various causes had been assigned to it.

The question, as it now stands, has been placed before the world in the work just published by the distinguished teacher above mentioned: "Lectures on Experimental Physiology applied to Medicine." Its appearance had been preceded by some controversial writings on the subject it embraces, and it is interesting to examine and ascertain what position its doctrines occupy at present.

It was in an Inaugural Thesis, published in December, 1843, that Mr. Bernard first made known his researches on the production and destruction of sugar in the system. Like many other discoveries in science, the first facts on this subject presented themselves during the investigation of other phenomena. It is in this manner that inductive and experimental physiology has so much and so frequently contributed to the developement of medicine.

Chemistry had insisted that all the principles met with in the organism, must be traced from substances, capable of corresponding metamorphoses by the formula or in the laboratory of the chemist; and that thus, every organic compound to be formed in the body, should be a derivative by oxidation or by re-arrangement of atoms, of that which had been formed exteriorly; that the animal fabric was capable only of destructive metamorphosis, and that the saccharine principles occurring in the economy owed their origin only to the vegetable constituents of the food. It was from this, that Prout assigned the presence of sugar in diabetes mellitus to the imperfect transformation and assimilation

of the starchy elements of nutrition, considered it a defect of the digestive functions, and called away the attention of pathologists from the kidneys, which had been previously accused in this disease. These organs which had so frequently been found inflamed, diseased, and even destroyed, worn out by their efforts to eliminate a substance that was in excess in the blood, had been regarded as primarily at fault, and responsible for all the phenomena of the disease; the other organs were not made the objects of proper examination. It is physiology which has taken up the task and done so much for this interesting subject; but it is yet incomplete; the knife of the experimenter, it is true, has laid bare the seat of the production of animal sugar, and has shown us where it always exists in the system in health; it is the scalpel of the pathological anatomist, now, that must continue the work, and point out the lesions in the diseases with which the subject is associated; one which appears to be singularly deficient in records of post mortem observations, as well as intractable by known remedies; and it is to call attention to these circumstances that our remarks are made.

As regards the internal production of sugar in animals without the intervention of saccharine or amylaceous food, that a substance, previously supposed to be elaborated only in the plant, should be formed in animals, does not, at the present day, appear singularly remarkable, since the production of cellulose, a vegetable tissue in the structure of lower animals, and even in the nervous system of man is now a well-admitted fact. (Virchow.)

The first proposition announced by Bernard was, that in dogs which had for months been fed solely on meat and then killed, glucose (grape sugar) was found in the blood of the hepatic vein, and in the liver, but could not be detected in the blood of the vessels leading to that organ. Hence, he drew the deduction that the liver is the laboratory for its production, as well as the demonstration of the fact that sugar can originate in the organism, independently of an external source, that is, of sugar-yielding vegetable aliment; that it is a true secretion, and as later experiments have proved, subordinate, as the functions of other glands are, to the nervous influence.

The points then to be inquired into and determined were: What proofs have we that the liver is the sole producing organ?

what are the materials and conditions for its production? what physiological office does glucose perform in the economy? what are the causes of its accumulation in the blood and appearance in the urine in disease? and from these facts to form a rational plan of treatment of diabetes mellitus.

In 1778, Cowley first isolated the saccharine principle of diabetes; but it was not until 1815, that the demonstration was made by Chevreuil, that this sugar is identical with the sugar of fecula or grape sugar, belonging to a class which form a separate group from those which are represented by cane sugar.

These first are all recognizable by the caustic alkalies, by the addition of which and heat, they are converted into peculiar brown acids; those of the other class are not thus affected. This, however, is not a good test in fluids containing organic substances, as the blood and urine.

Trommer ascertained that glucose has the property of decomposing certain salts of copper in the presence of an alkali. If the liquid be heated, the oxide of copper is precipitated of a characteristic yellow orange, or brick dust color; he ascertained, also, that this does not occur with cane sugar.

A modification of this, consisting of a solution of the double tartrate of copper and potassa has been proposed by Barreswill, and is the one recommended and used by Bernard in his experiments. If this be added to a solution of glucose, it loses its blue color, and on heating the liquid, there is formed the yellowish red precipitate of the oxide of copper. The quantity of the test fluid to be added, should be in proportion to the amount of glucose suspected to be present, in order that reduction of the salt of copper be complete and the characteristic color obtained.

It should be borne in mind, that cane sugars are by acids converted into grape sugars, so that if an acid have found its way into the mixture, this test would reveal also the occurrence of of sugars of that class.

Charcoal is the best means of decolorizing any fluid and freeing it of all subtances, as uric acid, dextrine, &c., which might interfere with the reactions of the double salt of copper. In consequence of the objections that have been made to these tests, and which will be considered shortly, it is important and desirable to employ the fermentation test, by the addition of yeast to

the suspected liquid, for the production of alcohol and carbonic acid from the glucose present.

The presence of sugar in the liver is now an undeniable fact. It may be made apparent with facility in a decoction of the viscus, by any of the above tests, as has been frequently done by the writer. This glycogenic function would appear to exist in all the animals as well as man, thus far examined. Its proportion in the physiological state does not vary much. The mean is about 2 per cent. for the entire weight of the liver in mammalia and birds. It is said to be less in reptiles, and still less in the mollusca. Its existence may easily be determined in these last, by using several livers, making a concentrated decoction, decolorizing by charcoal or sulphate of soda and employing the tests of copper, fermentation, &c.

The animals must be killed in full health, as in chronic diseases, shortly before death, this function of the liver, like those of the other organs, ceases. It is for this reason that the livers of subjects in the anatomical rooms will not yield glucose. Mr. Bernard has had the opportunity of examining the livers of six persons who had met with violent deaths whilst in full health, executed criminals, &c. In all of them, the liver yielded glucose, which could not be detected in any of the other organs.

The liver, then, is the only organ furnishing this substance, and the question naturally arises, whence does it obtain it? does it originate in its tissue, or is it carried there in the blood? The inference drawn from the capital experiment of Bernard, viz., that the blood of the vessels entering the liver did not yield glucose, whilst that leaving that viscus yielded it freely, and which would seem to point out, with precision, the starting point of this principle, has not been admitted without modification and opposition.

According to the experiments of Poggiale, a dog fed on a mixed diet, or a diet of bread alone, and then killed, gave evidences of glucose in the blood of the portal vein, in that of the inferior cava, and even of the arteries. The proportion of glucose in the hepatic vein is said not to have, in any degree, exceeded that in the vessels entering the liver. In this case, this substance would appear to have found its way from the stomach and alimentary canal into the circulation, for in animals fed exclusively on flesh, no glu-

cose could be detected in the vena portarum by the same observer. In experiments performed by Mr. Bernard and Mr. Leconte, and at which the writer assisted, glucose did not appear in the blood of the portal vein, if care were taken to prevent its *reflux from the liver*, whilst it was abundantly furnished by that of the hepatic vein. The animals in these, as in the above case, had been on an exclusive diet of flesh for many months. This would appear conclusive as to the liver originating the glucose. The first experiments of Mr. Poggiale confirm what might have been expected; that in animals living on a mixed alimentation, the food will also furnish sugar in the blood. Hence there would be for man a double source. In herbivora, it is stated by Mr. Colin, of Alfert, that sugar exists in the blood, in the chyle and in the lymph, the saccharine portions of the food furnishing them to the portal veins and chyliferous vessels. He further adds, however, that in the carnivora proper he found that an animal diet furnished sugar to these vessels during digestion?

The strongest opposition to Mr. Bernard's experiments has recently been made by Mr. Figuier, Assistant Professor in the School of Pharmacy in Paris, and it is to him that his recent rejoinders have been directed. This gentleman had asserted, as a fact, that the presence of albuminose may interfere with the precipitation of oxide of copper in a fluid containing glucose, (the test of Bernard and Barreswill,) and that it is the existence of this principle in the portal blood, which masks the presence of grape sugar. He precipitates the albuminoid substances by alcohol, and recommends the addition of a small portion of acetic acid, to neutralize the carbonate of soda present. With these precautions, he insists that glucose can be detected not only in the blood of the portal vein, but in that of the entire system. Mr. Figuier says that he has verified these facts by more than thirty carefully conducted experiments; and he further adds, that for equal weights, the liver can be said to contain scarce twice the amount of glucose to be found in other parts of the body. According to this gentleman, in Bernard's experiments, the flesh of beef and mutton on which the animals were fed, contains glucose, and thus the substance sought was administered to them in their food.

True, it is, that in Mr. Bernard's doctrine of digestion, albuminose derived from the digestion of meat, is conveyed by the portal

vein to the liver, and is there reconverted into albumen, which does not, like albuminose, mask the reactions of the salt of copper and potassa with glucose and heat.

The liver, in the opinion of Mr. Figuier, is a reservoir in which animal sugar is stored up, not manufactured; and he strengthens his view by the following experiments:

A dog, which had been fed exclusively on animal food, was killed, *two hours* after the last meal; that is, during digestion. The blood entering and leaving the liver was examined, that of the portal vein gave for 100 pts. 0.248 of glucose; that passing from the liver gave but a trace. Repeating the same experiment *four hours* after the last meal, that is, when digestion was more advanced, analysis gave:—

For the blood of the portal vein, (100 pts.) = glucose 2.31
" " veins beyond the liver, = " 0.304

In an animal kept fasting, the blood of the portal veins gave not a trace, whilst that of the hepatic vein was rich in glucose. In the circle, then, of the circulation, says this observer, the supply obtained from the liver is consumed in respiration, and a fresh amount is furnished to the blood each time that it passes through that organ.

The discrepancy in the results obtained show the want of additional tests for glucose; the fermentation test is useful only where there is a notable proportion of this substance present. The changes of color exhibited by the copper test may be more or less intense, and give rise to uncertain conclusions. The commission appointed to decide on the question have pronounced against Mr. Figuier's analyses, as he did not make use of the fermentation test as well as the others in his experiments.

Longet also has found that the presence of completely digested albuminoid substances masks the reaction of the copper test with glucose. He says that he detected, by the fermentation test, this principle in the blood of the portal vein in cases where it was not revealed by the double salt of copper and potassa, and even when it had been detected in the alimentary canal and in the hepatic vein by the ordinary means. Hence, he considers that some further method of experimenting must be found before the glycogenic function of the liver, based upon the evidences

furnished by the present chemical reagents, can be unqualifiedly adopted. His language is:—"When the product of the transformation of an azotized aliment by the gastric juice is in considerable quantity, and the glucose in small proportion, the tartrate of copper and potassa, potassa, the polarimeter, alcoholic fermentation, in a word, none of the means now in use can demonstrate positively the existence of this saccharine principle."

Mr. Lehmann has come forward to support Mr. Bernard's view by his own experiments. He invariably makes use of the fermentation test in addition to the others, and finds that the blood of the portal vein in dogs that have been kept fasting or fed only on meat does not contain the slightest trace of glucose; a vegetable diet causes its appearance in these vessels in small proportions only. In horses fed on grain the proportion was also very small, but in both horses and dogs glucose was abundant in the blood of the veins leaving the liver. The other experiments of Mr. Lehmann show also conclusively that sugar disappears in the blood, and that the further we proceed from the liver in the circulation, the proportion decreases. It is most abundant in the hepatic vein and in that of the vena cava above the junction of that vessel, but in the right side of the heart it becomes intermingled with the blood of the vena cava descendens, which contains no sugar, and thus the proportion is there diminished. It can then be traced into the lungs, where it generally disappears.

The liver then is a secretory organ having a double function, that of producing bile and also sugar. The secretion of sugar is a true secretion, not an excretory function, as that of urea by the kidneys; for in these organs urea is found in the blood entering them but not in that which flows from them, the reverse of that which occurs in the liver with regard to glucose. That this secretion may be considered as under the control of the nervous influence is well shown by the experiments of Bernard, and since repeated by others, that if the floor of the fourth ventricle be punctured in an animal an artificial diabetes mellitus is established.

This well known fact is most important to be borne in mind, as it may afford a clue to the comprehension of the pathological state of this function. There must be in health an equilibrium between the production and the destruction of glucose. If more

is supplied to the lungs than they can get rid of, it will appear in the blood beyond the pulmonary apparatus, and be removed from the general circulation by the kidneys. This is frequently and even constantly occurring in the normal state. There is after a mixed meal the fabrication of glucose to a greater amount than during fasting, and more than is consumed in respiration, and during digestion we may have a temporary glycosuria. Patients should be examined then whilst fasting, and the examinations repeated and an average result obtained before we can determine on the amount of disease. The strong sympathetic relation between the glycogenic function of the liver and respiration is abundantly proved. If the pneumo-gastric nerve be cut and the extremity nearest the brain be irritated, there takes place an excessive production of glucose as indicated in the urine; there is an impression made on the cerebral centre, and reflected through the fibres of the sympathetic accompanying the vessels of the liver, which influences the production of this principle. The impression made is similar to the excitation arising from the demands of the lungs.

This sympathy, though demonstrable, is not readily explained. We see a confirmation of it in diabetes mellitus, in which death so generally occurs from pulmonary apoplexy or phthisis. In the triad of the liver, the lungs and the nervous centres, it is difficult to isolate the incipient phenomena so as to ascertain the starting point of disease. There is an unfortunate dearth of cadaveric reports on this subject. The disease in question is not a common one, its commencement is often insidious and unheeded, and it is rare that we have the consecutive phenomena described *ab initio*. That lesions of the nervous system may give rise to diabetes mellitus has been observed by Doctor Goolden, who has collected some very interesting cases, one in particular, in which it followed a blow on the back of the neck. This and the others were relieved by directing remedies to this portion of the system; and a case has fallen under our notice in which a patient first exhibited symptoms of a spinal adynamia, followed by pulmonary disease and asthma, and in whom the urine subsequently gave evidence of abundance of glucose.

The medulla oblongata was in this instance apparently the first at fault in the chain of morbid phenomena. More extended patho-

logical observations may throw light on the order of events in the physiological problem. That the causes assigned by Prout are insufficient, and that the liver can produce glucose from the nitrogenized material conveyed to it by the blood is unquestionable. Lehmann's experiments would show that in any event the amount of sugar conveyed in the portal vein under any diet is but a fractional part of that which leaves the liver, and corroborates Bernard's doctrine.

In the dearth of pathological information, it is gratifying to be able to refer to some facts recently put forth by so reliable an authority as Andral. In eight autopsies of persons who died of diabetes mellitus, he found the liver redder and more full of blood than usual; it was also more uniformly red, showing the increased functional activity of that organ. He adds the following interesting statements as regards the results of dietetic treatment. Entire abstinence from food will diminish and sometimes suppress the appearance of sugar in the urine; but he found also that a change of diet, whether it be the addition of vegetable aliment or its entire subtraction, will at first produce a reduction of the amount of sugar, but this will afterwards regain its usual standard.

Lehmann having found that the blood leaving the liver, and which abounded in sugar, was deficient in fibrin and hæmatosin, considered that glucose was formed at the expense of these nitrogenized elements. He succeeded in obtaining from hæmatosin a substance which gave reactions similar to glucose, which confirmed him in the opinion. Frierichs had a similar hypothesis. The opinion that fatty matter furnished the material is no longer sustained. Rollo withheld from his patients all food not of this nature; the sugar was diminished, but the individuals subjected to this regimen were in similar conditions with those who practised perfect abstinence. There can be no doubt but that sugar-yielding food will contribute to the glycogenic production, though not to the extent previously imagined. Bernard states that if amylaceous or saccharine nutriment be administered to a dog, the proportion of sugar in the liver will not be increased materially, but there will appear an opalescent emulsive fluid, the result of the metamorphosis of the starchy materials.

It does not necessarily appear, however, that in disease the

saccharine matters in the duodenum, the result of digestion, may not pass into the portal vein, be unaffected by the liver, and be thus added to the amount secreted by that organ.

So late as the 24th of September of the present year, Mr. Bernard has brought forward a paper in which he discards all the hypotheses as regards the elements in the blood, &c., from which sugar is produced in the liver. He says that it is the *tissue* of the liver itself which furnishes the materials, and adduces as proof the following experiments. Having carefully removed the liver of a healthy dog, he fastened a water cock to the trunk of the portal vein and allowed water to flow with force through the ramifications of that vessel, and thus through the organ itself, escaping by the hepatic veins. The first portion which passed through yielded sugar abundantly; this was continued until the fluid became colorless and ceased to yield sugar; in fact the liver was completely washed out. The decoction of a portion of the *liver* thus treated gave no sign of sugar. The other portion of the viscus was laid aside for 24 hours, at the end of which time a stream of water was passed through it, which gave abundant evidences of glucose, both by the copper test and fermentation.

This experiment would prove, says the author, that in the liver, in its physiological state, "there are two substances, viz: 1st, Sugar very soluble in water, and which is carried off with the blood in washing. 2d, Another substance sufficiently insoluble to remain in the hepatic tissue after this had been deprived of its sugar and of its blood by protracted washing. It is this last substance in the liver, laid aside, which becomes by degrees converted into glucose by a species of fermentation."

He has by further experiments showed that this matter is insoluble in alcohol and ether. He was unable to detect it in the blood of the portal veins. He considers this discovery as of great value in settling the glycogenic function of the liver, and concludes by the following remarks:

"I wish to show that the sugar formed in the liver is not produced (d'emblée) at once, without intermediate processes in the blood, but that its presence is constantly preceded by a special material deposited in the tissue of the liver, and which immediately precedes its formation." He calls the attention of chemists to the intermediate formations.

Of the uses of glucose in the animal economy there is no absolute demonstration. Its disappearance in the lungs informs us that it is consumed in respiration. Its transformation into lactic acid is readily conceivable, but is simply hypothetical. That the lactates become converted into carbonates, which are again decomposed by the pneumic acid of the lungs, the CO^2 being set free, is the modern doctrine of physiology. Like the other hydrocarbons and fatty matter, its office must be intimately connected with the production of animal heat.

It is a fact worthy of observation that the livers of birds, (particularly the palmipedes,) which abound in fat, contain also a very large proportion of glucose. In human fatty livers, glucose is always in excess. From the preceding review of the most recent facts and investigations on this subject, we may gather the following conclusions :

That sugar is a normal product in man.

That this principle is secreted in the liver, and that it is a normal function of that organ.

That the source of its supply is from nitrogenized elements.

That the food furnishes it also to the system.

That in the glycogenic function there is a sympathy of relation between the liver, the lungs and the cerebral centre.

That in the disease called Diabetes Mellitus, the equilibrium of the production and destruction is disturbed, and that any one of these three structures may be at fault ; and that it is to one or more of them that our remedies must be directed.

That the experiments of Lehmann, Bernard and Andral will warrant the careful allowance of small portions of vegetable food in this disease, and thus relieve our patients from one of the most distressing and trying attendants of the present mode of treatment.

That the labors of the physiologists, and above all, of Mr. Claude Bernard, have paved the way for a better understanding of diabetes mellitus, by demonstrating the condition of the glycogenic function in the state of health ; but that close and more extended pathological observations are called for to render his researches available to the physician for a successful plan of treatment of a disease which is rare, but has thus far proved intractable.

Report of a Case of Malformation of the Colon and Hand.

By Wm. Walters, Frederick, Maryland.

On September 17th, 1855, I was called to Mrs. ——, then in labor, and delivered her of twin daughters between 12 and 10 o'clock A. M. The first-born, who is the subject of this report, presented the appearance of an eighth month child, very feeble, and was soon after birth almost suffocated with bloody mucus. This continued throughout the day, but late at night the bloody appearance ceased, the discharge becoming white. Her limbs and face were livid and her left wrist and hand presented a singular malformation. The metacarpal bone and phalanges of the thumb were entirely absent, and the hand was bent upwards upon the wrist at a right angle, so as to resemble very much a misplaced foot.

On the 18th she appeared stronger and better, breathing much easier. The nurse called my attention to a tumor slightly protruding from the os vaginæ, and to the fact of the bowels not having been moved, although the usual aperient had been given. I attempted to give her an enema of lard and water, but found that it flowed back without being at all tinged with meconium. I now introduced a flexible catheter into the rectum. It passed up easily about four inches, but was then arrested, and an attempt to inject through it resulted as before. Both these attempts induced strong straining and bearing down efforts, with discharge of urine and further protrusion of the tumor at the os vaginæ. The parents positively objecting to any attempts at surgical relief, and believing the case to be one of imperforate colon, I now desisted from further efforts to open the bowels, and stated to them that I thought death must soon ensue. During the day the child vomited four or five times, and made frequent straining efforts and bearing down of the perineum.

19th. Occasional vomiting, straining frequent, but no evacuation of the bowels. Had a convulsion last night.

20th. Symptoms the same.

21st. Died at 2 o'clock A. M., having lived ninety-eight hours.

Autopsy made eight hours after death.

The os vaginæ being entirely closed by the hymen, the ap-

parent tumor projecting externally between the labia was found to be produced by a fluid retained in the vagina.

On opening the abdomen the uterus was found pushed up above the umbilicus, by the distended vagina; the uterus was enlarged and dilated, the greatest diameter of its cavity being half an inch, and the greatest length one and a quarter inch.

The vagina was three inches long and one inch in diameter, being thus dilated by the fluid referred to above. This was of a creamy color and consistence, and under the microscope appeared almost entirely composed of squamous mucous epithelium.

But the greatest point of interest and evident cause of death, was the anomalous conformation of the intestines. Six and a half inches below the cœcum, the colon terminated in a cul de sac. The small intestine and this portion of the colon were normal in size, direction and condition, except their peritoneal surface, which exhibited high arterial engorgement and in some places inflammation. They contained meconium, with which the small intestine was slightly distended.

The terminating point of the colon was a little below the commencement of its transverse portion, and adjacent to this point it was found again commencing in a cul de sac, and extended thence uninterruptedly seven inches to the sigmoid flexure. The whole extent of this portion was contracted almost to a cord, and was entirely empty. The rectum was also empty and contracted at its upper portion. Its length was five inches.

The surviving twin-sister is a fine healthy child, with no defect or malformation whatever.

On the Use of the Tinct. Ferr. Chlorid. in Uterine Hemorrhages.

By Dr. Frederick Schreier, Physician in Hamburg.

(Translated for the Examiner.)

Experience having shown that all the means generally used for the suppression of uterine hemorrhages—especially when violent—were inefficient, I was induced, after trying various remedies, to employ the liq. stypticus loofi. Not only was my first experiment, made thirteen years ago, attended with success, but I have lately had frequent opportunities of collecting from my colleagues proofs of still more favorable results from the use of this medi-

cine in their practice. The history of two cases will best illustrate the practical working of the liq. ferr. chlorid.

In the beginning of the year 1842 I was called to visit the midwife B., in Altenwerder, Hanover. This woman, 26 years of age, of delicate constitution, complained of violent pain in the right lumbar region. On examination I observed a swelling, which bore all the characteristics of the first stage of an abscess. After applying the usual means for producing suppuration, it opened and was healed. About 14 days afterwards, Mrs. B. being sent for to attend a woman about to be confined, was obliged to cross the ice on horseback. The horse broke through the ice and the woman fell into the water. On the next day a violent hemorrhage took place, to stop which she used all the well known means, but in vain. Called to see her, I ordered tinct. cinnamon 1—3 teaspoonful every quarter of an hour; with no effect however, as the bleeding continued undiminished. A threatened mis-carriage could not now be mistaken. Weak, irregular labor pains had commenced, and I was satisfied that the fœtus would have to be expelled from the uterus. I now prescribed: ℞. Secal. cornut. gr. x., rad. ipecac. gr. j., alumin. crud. gr. jv., sacch. alb. gr. vj., M. f. pulv., to be given every hour. I ordered the most perfect rest, horizontal position, and laid a cold sand bag over the pubic region. After this the hemorrhage became less.

On the evening of the same day I was informed that not only had the bleeding not ceased, but that it had become much more violent and the woman very weak. I at once ordered: ℞. Decoct. rad. krameriæ ʒvj., tinct. cinnam. ʒß., syr. cinnamon ʒj., acid phosph. a. ʒij. M. D. S., one teaspoonful every hour, and hurried, suspecting great danger, to the sick woman, whom I found suffering from severe hemorrhage. After examining the mouth of the womb (which was so contracted as to render the insertion of the finger impossible) I applied restoratives (besides injections of cold water, alum and vinegar) after which the bleeding ceased; but for this time also only for a short period; for in the night, toward 12 o'clock, a message was sent to me that the bleeding had again commenced, the patient being entirely exhausted and dying. Upon this I resorted to the energetic measures which I had before used in similar cases with such

favorable results, namely, injections of diluted liquor stypt. loofi. (about 50 drops of liq. stypt. l. in 3—4 oz. of water). Finding after this injection that no sensation of heat was felt in the womb, that the bleeding was unchecked, and that no perceptible contraction of the uterus had taken place, I now gave, instead of 50, 80 and finally 100 drops in 3 oz. of water. The wished for result at once took place. The passage of black coagulated blood with the fœtus followed the injection. The fœtus appeared about 12—13 weeks old, which accorded with the woman's reckoning. Afterwards occurred gastrosis with tympanitis, &c., &c., which was arrested by the appropriate means, and the patient soon completely recovered. She afterwards described her sensations when the above means were used in the following manner: "Upon the first injection I felt a slight warmth, but upon its being repeated after being made stronger, a most decided heat was experienced in the womb, which increased to a burning sensation, accompanied with a peculiar feeling of motion there, after which I was not aware of any further loss of blood."

I will now relate another case equally interesting.

Mrs. F., at Finkenwärder, 40 years old, of strong constitution, choleric temperament, was seized with a hemorrhage. She summoned an esteemed colleague, Dr. N., in N., to her assistance. After trying all the known and valued means in vain, the patient called me in. I resorted again to the injection of liq. stypt. loofi., for the patient had now become almost anæmic, and I had again the satisfaction of witnessing the successful results of its action. After the third injection—in which were 100 drops of liq. stypt. l.—the bleeding ceased. In consequence of the great loss of blood, general dropsy afterwards took place. The cause of the dropsy was clear, and I treated it therefore with iron and other restoratives, and soon checked it. After the course of three years the woman was again pregnant, and was delivered of a living, mature child.

In cases where paralysis of the uterus occurs, and the power of contraction is wanting, I can also recommend to my brethren, as a safe and effective measure, the use of the liq. stypt. l. In the last year of my practice I used the liquor ferr. chlorid. also for a violent hemorrhage in a case of placenta prævia, where rapid delivery could not be effected, owing to insufficient dilata-

tion of the mouth of the womb. I applied it in the following manner: I took a compressed sponge (sponge tent) which was cut in the form of the opening of the mouth of the womb, saturated it with the liquor, and introduced it as high as possible into the mouth of the womb, upon which not only was the bleeding checked, but after the course of 6—12 hours, in consequence of the distention, the dilatation was so increased as to allow me to introduce the whole hand and complete the delivery without any injury whatever. And indeed, in my practice I have never in consequence of an artificial delivery lost a single patient. In uterine cancers, where profuse bleeding frequently occurs, the same remedy can also be recommended.—*Monatsschrift für Geburtskunde und Frauenkranpheiten, June,* 1855.

BIBLIOGRAPHICAL NOTICES.

A Treatise on Medical Jurisprudence. By FRANCIS WHARTON, Author of "A Treatise on American Criminal Law," etc., etc., and MORETON STILLÉ, M. D., Lecturer on the Principles and Practice of Medicine in the Philadelphia Association for Medical Instruction. Philadelphia: Kay & Brother. 1855. 8vo. pp. 815.

Medical Jurisprudence, "the science which teaches the application of every branch of medical knowledge to the purposes of the law," would seem to be a subject of trivial importance were we to estimate its value by the little time and attention generally bestowed upon its study. Its claims upon the profession are nevertheless among the very strongest. We grant that its skilful or inefficient exercise may not affect the community so directly for good or evil as may that of some of the other accomplishments of a medical education. Still, it can not be denied that the practitioner of medicine is at all times liable to be called upon for his special evidence in the elucidation of questions which come home to the business and bosoms of individuals and communities as nearly as life and health, and in which every

thing may depend upon his acquirements in the kind of training which medical jurisprudence alone can adequately give.

The presumed and actual ignorance of juries of all professional knowledge, as a matter of course, inclines them to confide in the testimony of an expert, who is capable of conveying clearly and precisely the information looked for, and who is able to maintain his position, whether as to fact or opinion, in the face of an astute and learned cross-examination. On the other hand, for the same reason, nothing is more lame and hopeless in its practical influence on the witness stand, or destructive to the moral weight of medical authority, than the blundering self-contradictions, hesitation and confusion, not to say positive stupidity as to the point at issue, with which even eminent members of our fraternity too often annoy the court and mistify an already puzzled jury. How often might we escape these humiliating exhibitions, and how easily repel the sneer which is only too well earned by many otherwise deserving members of an unappreciated calling, if such works as the excellent one before us were more frequently consulted! How much better for us all were they made the text books to a bonâ fide course of teaching in our regular curriculum, instead of being left, as they almost universally are, to the caprice of the student, as matters of mere elegant amusement.

Our readers can be at no loss to recall notorious as well as minor instances of the wretched consequences of a want of this kind of medical instruction, or of an amount of self-preparation which the treatise of Wharton and Stillé abundantly provides for and brings within the reach of every intelligent practitioner of law or of medicine.

There are men of high standing in each vocation who are disposed to regard the position here assumed for the study of medical evidence as unduly exalted. They wish to deny to it the name of a distinct department of science, and accuse it of increasing the very difficulty and embarrassment which it was intended to do away. The most conclusive answer to such objections would be rendered, as it has been time and time again, by the argumentum ad hominem in a court of justice. We suspect that few who have tried it in such a spirit will deny the truth of Mr. Taylor's remark, when he says that he never knew such

"indifference manifested by one who had once been summoned as a medical witness to a court of law."

But it is to the doubting members of the two professions that we wish especially to recommend the book before us, as one which is, of all others, the most likely to satisfy their scruples, if not to change their views. It will command their attention as the joint production of able representatives of either body. It meets the most urgent want of sceptics of both sides, inasmuch as each of the two authors was peculiarly well qualified by character of mind and by long previous study, not only to discuss lucidly the parts assigned to him, but to assist and guide his fellow in the duty of co-ordinating the different topics—of giving to each point of law or medicine, respectively, the cast required to make it useful and acceptable alike to lawyer and physician.

The best idea, however, of their objects and the general arrangement of their labors, may be derived from the surviving author's preface, two paragraphs of which we here subjoin.

"The two points which were mainly before the authors of the following treatise when they entered upon its preparation, and the hope of reaching which formed their chief inducement in approaching a topic which has already been in other respects so ably and fully discussed elsewhere, were first, the incorporation in its pages of the results of late continental, and particularly French and German research; and secondly, the bringing together stereoscopically—if the metaphor can be permitted—of the Legal and Medical points of vision, so that the information required by each profession might be collected and viewed at the same time and within the same compass. It was felt that in the usual range of medico-legal exposition, there was a great deal that, though interesting to the medical man, is unnecessary to the legal practitioner; and, on the other hand, it is equally clear that there are many points upon which the latter needs information, which the former, either from inadvertence or what would be to him their extreme simplicity, may forbear to touch. The converse also is true, viz., that the legal writer who undertakes such a work, except in subordination to medical advice, may exhibit very satisfactorily the necessities of legal practice, but will fail to supply the information by which these necessities can be met.

"These two points, viz., the absorption of recent medico-legal research in France and Germany, and the union of the medical and legal stand points, it was here hoped to reach, not so much by a concurrent authorship of each page, as by a general preliminary comparison of views and adjustment of material by the two writers by whom the task was undertaken, followed up by a division of the subject matter between them in subordination to the plan previously agreed upon. In pursuance of this scheme, the second, third, fourth, and fifth books,—

viz., those on the Fœtus and New-born child, on Sexual Relations, on Identity, and on the Causes of Death, were assigned to Dr. Stillé, with the exception of those sections in the two former which concern the legal relations of gestation, abortion and rape; while to the present writer fell the preparation of the first and sixth books, embracing Mental Unsoundness and the Legal Relations of Homicide, together with the general disquisition on Indicatory Evidence with which the latter book concludes."

The first book, containing two hundred and twenty-eight pages, is devoted to the consideration of Mental Unsoundness in its legal relations and interpretation. This portion belongs, as above stated, and as is already well known, entirely to Mr. Wharton. It was published separately some time ago, and has attracted a great deal of well merited attention. We have had the pleasure of noticing it at length in our number for August of this year, and therefore shall not dwell upon it now.

With regard to the technical merits of the other portions of the volume by the same author, we can not pretend to say much in a purely medical notice. We have been greatly interested, however, in the clear and ample expositions, and in the admirably concise and convenient method of arrangement and illustration, which characterize the chapters on the legal relations of rape and of homicide, fœticide and infanticide. The discussion of each of these topics is very materially aided and simplified by a preliminary analysis. These analytical tables are among the most striking and valuable features of the book, and will doubtless prove particularly serviceable to the medical inquirer. Some idea of their scope and tendency may be derived from the following brief extract, which introduces the article on the "Legal Relations of Rape."

§ "457. The points to which medical testimony is most likely to be invited, in prosecutions for rape, are the following:—

1st. Submission of prosecutrix.
   (1) From artificial stupefaction.
   (2) From ignorance of the nature of the act.
   (3) From mistake of person.
   (4) From fear.
2d. Prior want of character of prosecutrix.
3d. Subsequent suppression of the fact by prosecutrix.
4th. Extent to which coition was carried.
5th. Want of age of defendant.
6th. Want of sexual capacity of defendant.

The law on each of these points, will be now briefly considered.

1st. *Submission of prosecutrix.*

This may happen from either of the following causes:—

(1) *From artificial stupefaction.**

"§ 458. It makes no matter whether the drug was given for the purpose of producing stupefaction, in order that the rape might be effected on the female thus made unconscious, or whether it was administered for the purpose of causing sexual excitement, and thereby leading to a voluntary submission. It is rape in either case; the law being, that the overcoming of chastity, and the destroying of resistance by artificial means, is rape, when the offence is consummated. If the result of the dose be stupefaction, and if on the woman, thus become insensible, carnal intercourse be effected; it is rape, though the intention was merely to excite. Thus, where the prosecutrix was made drunk by the prisoner, who then violated her person, it was held in England, where, from the offence being capital, it is kept within its strict common law limits, that the crime was rape, though the jury expressly found that the liquor was given with the intent of *stupefying* not *exciting*.† And in this case, it was held, that where the insensibility is the defendant's act, and where the defendant knows that 'the act was against the prosecutrix's consent at the last moment she was capable of exercising her will,' it is rape. On this point agreed all the ten judges of England, constituting the final Court of Revision in criminal causes; and it was not required by the exigencies of the case that they should go further. Several, however, thought—and this view is in accordance with the analogous cases to be hereafter noticed—that the crime was consummated by the mere act of knowingly violating an insensible woman, whether the insensibility was produced by the defendant himself or not.

"§ 459. In the late prosecution of Dr. Beale, in Philadelphia, which has been noticed above,‡ the point was not made, and it was assumed by both sides, that carnal intercourse with a woman who was stupefied by chloroform was rape, though the chloroform was administered, at her request, for the purpose of facilitating the extracting a tooth. And if the law—and the cases to be subsequently noticed unite with the reason of the case in indicating that it is—it goes to establish the broad position, that rape is sexual intercourse with a woman, not *against*—as has been formerly said—her will, but *without* it." Pp. 351, 352.

Under the head of Corpus Delicti a great variety of deeply absorbing cases, new, as well as old, are presented in a most instructive manner. It would be easy to attract the most indifferent reader with passages from these strange annals of crime and its

* See the medical relations of this point, *ante*, § 439.
† R. v Camplin, 1 Car & K. 746; Wh. Cr. Law, (3d ed.) 514–15.
‡ See *ante*, § 445.

marvellous exposure. A quotation of a different kind, however, will serve our purpose better. It is in relation to "Marks of Violence," and may be presented as a pretty fair example, in its way, of the manner in which our author indicates the course to be pursued by medical investigators and observers.

"§ 1138. Marks of violence, in connection with the cause of death, have already been considered. At present they are only to be noticed in connection with the question of intent. It cannot be doubted that when a wound is found to have been inflicted in a secret or concealed part, which is inaccessible in sudden and passionate conflict, it bears a violent presumption of having been the result of design. Thus the wounds of which the Scotch historian tells as having been inflicted by forcing a heated iron into the fundament, could have been explained in no other way than on the hypothesis that to death was intended to be added concealment. In the same class may be enumerated the thrusting of a needle in the navel of an infant, running a sharp but slight intrument in the cavity behind the ear, dropping corrosive acids into the ear itself, and forcing molten lead down the throat through a tube; of each of which resorts the books give instances.* The principle on which the presumption of intent can be drawn from such cases is, that a person acting under the impulse of passion is much less likely to inflict a skilful wound, than one whose act is the result of premeditation.†

"§ 1139. Whether the wound was inflicted in self-defence or otherwise; whether it was self-inflicted, or inflicted by a stranger; whether the perpetrator of the crime was an expert or otherwise,—may also be deduced from the wound. And the direction of the wound may often be shown for the purpose of testing the validity of a defence. Thus, where the defence was, that the ground being rough and slippery, the prisoner stumbled, and both barrels of the gun had gone off by accident, the defence was confirmed by tracing the direction of the shot, which was found to be pointed upwards ‡ The difference in appearance between wounds inflicted before and after death, has been already considered.‖

"§ 1140. It is by medical testimony alone that the agency of the alleged violence, as a cause of death, is to be determined; and if the death was not accelerated by such violence, the defendant must be acquitted. Thus, in 1847, on a trial for manslaughter, the surgeon who had attended the deceased, stated that on examining her body he had

* Mittermaier von Beweise, 402; Demme's Annalen des Criminalrechts, vol. iii. p. 215; Bauer, Theorie des Anzeigenbeweises; Henke Darstellung, sec. 99; Blanci de Indiciis, Venet, 1545; Reinhardt de eo quod circa reum ex Præsumpt. Convinc. et Cond. Just. &c. Erford, 1732; Heinroth in Hitzig's Zeitschrift, n. 42, p. 257.

† Presumption from gun-shot wound, see *ante*, § 811.

‡ Watson on Homicide, § 246. ‖ *Ante*, § 810--816.

found the mark of an old wound on her head, and a slight bruise on one of her thighs; but he further stated that he made a *post mortem* examination of the body, and that his opinion was, that the cause of the deceased's death was confirmed consumption, her lungs being tuberculous, and that it had not been accelerated by violence, but was wholly attributable to natural causes. The defendant, under the direction of MURPHY, Sergeant, who consulted with Lord Chief Baron POLLOCK, was acquitted.* But it is no defence that the deceased was laboring under a mortal disease, if death was accelerated by the defendant's violence;† and this, no matter how remote the cause, if the intention was to commit an assault, and death resulted."‡ Pp. 748, 749.

Book second, which is the first of those assigned to Dr. Stillé, is divided into six chapters. These are occupied respectively and successively with the "Signs of Pregnancy," "Delivery," "Duration of Pregnancy," "Superfœtation," "Abortion or Fœticide," and "Infanticide." The third book is divided into three chapters, which treat of "Doubtful Sex," "Sexual Disability" and "Rape." Book fourth contains but one chapter, which is devoted to the "Identification of the Living or Dead."

The fifth and last book of the medical series, discusses the "Questions Relative to the Causes of Death." It is divided into two parts, the first of which includes "Poisoning" and the various poisons, in numerous chapters under the usual heads. The second of these parts continues the course with the consideration of the "Other Forms of Violent Death," such as Wounds, Burns, Spontaneous Combustion, Heat, Sun Stroke, Lightning, Cold, Starvation, Suffocation, Strangulation, Hanging and Drowning, and completes it with a chapter on "Medico-Legal Examinations."

Even a slight examination of these pages will satisfy those who are familiar with the subject, that the promise of the preface is abundantly sustained in them, no less than in the essays of his accomplished colleague. Indeed the greater scope and variety of available material in the French and German publications required by Dr. Stillé, has enabled him to render the peculiarity aimed at in regard to the results of European continental research, especially remarkable in his share of the labor. Nearly every page gives proof of the industry and discrimination with

* R. *v.* Conner, 2 C. and K. 518. See also R. *v.* Crompton, C. and Mars. 597
† State *v.* Morea, 2 Ala. 275. ‡ Wh. C. L. (3d edit.) 363.

which he has explored and gleaned from those invaluable and ever-growing fields of scientific cultivation, which are beyond the reach of ordinary students. But in thus gathering and presenting to his American readers a digest of experience, which, although hitherto comparatively little known, is unsurpassed in authority and varied interest, he has by no means lost sight of British and American cases and results. The cream of the whole subject is laid before us in a manner at once simple and unpretending, yet with a brevity and logical precision, an unusual cautiousness, and a judicious and manly independence of opinion, which decidedly increase the authority of the book, while they add to its freshness and interest as a truly original production. As far as we are able to determine, it is perfectly reliable in its doctrine and citations, and may be said to have been brought, in some of its cases and recently established practice, in advance of any other of the standard manuals yet published in our language. We shall have an opportunity to recur again to the history as well as the peculiar features of the essay of our departed friend, which render it a sadly cherished memento of a career so full of promise and so painfully cut short in the act of its fulfilment. Meanwhile we take pleasure in submitting a few chance extracts which may serve as examples of his ordinary style and mode of handling his topics. In relation to quickening in pregnancy, he dwells upon an important but still unappreciated truth in the following language:

"§ 283. The undue importance attached to quickening, from the earliest times, arose from an error which modern science would long since have consigned to oblivion, had it not been fatally incorporated into the laws of various countries. It was supposed that the fœtus became endowed with vitality at a variable epoch after conception, and that quickening was an indication of the moment at which it became thus animated. Such an error, explicable in the infancy of physiological science, by an inadequate knowledge of the development of the embryo, confirmed by absurd ecclesiastical canons, and handed down from one criminal code to another, should now, when ignorance is no longer excusable, disappear from our penal system. To whatever cause the act of quickening may be attributed, its explanation is not dependent upon a solution of the question relative to the precise moment at which the child becomes endowed with life. If it be due to the first motions of the child, perceptible to the mother, it is merely an indication of its *first muscular movements;* and if it is caused by the sudden rising of the uterus from the pelvis, it evidently has a still more dis-

tant connection with the phenomena of life. No serious argument is required to prove that the fœtus, in its embryonic condition, is a new being, living by its connection with its mother, dying when this is destroyed. However rudimentary its form, it is not an inorganic body, constituted by the casual aggregation of atoms, but a living creature, from whose undeveloped lineaments a perfect human shape is to be evolved. A pulsating heart, and a nervous tract, are among its earliest recognizable elements. Reason and observation equally declare its essential vitality." P. 232.

The following curious history of a case of feigned delivery, reminds us of the somewhat similar, but more honest, management of the barren wife of a patriarch of old.

"§ 301. *Feigned delivery.* Delivery may be feigned from a variety of motives, into which it is not necessary for us to enter. A medical inspection can hardly fail to expose the deceit, and usually the collateral proof is sufficient. We have abridged the following case of feigned delivery, on account of the wonderful ingenuity with which the imposture was conducted. Dr. Albert relates that he was called upon to see a poor girl of twenty-one years of age in her last illness. In the presence of the physician and clergyman of the district, she gave the following narrative and confession. Some eighteen months previously she had entered the service of a married couple as housemaid. Her master, who was young and handsome, and assumed the title of Baron, had no children. He succeeded, by tempting presents, in overcoming her virtue. He then represented to her, that an important inheritance depended upon his having an heir, but having been married five years, and his wife still proving unfruitful, he had no longer any hope of having children by her. He then proposed to the girl, that in case she should prove with child, and would allow him to cause it to appear as his own legitimate offspring, he would not only give her a considerable sum of money, but would also let her remain in the house with her mistress, in order that she might be always near her child. She accepted the proposal, and as soon as she found herself to be pregnant, the preparations were made to carry out the projected imposture. The girl remained in the house, living in the most retired manner, while her mistress played the part of a lady in an interesting condition. She introduced wool and folded napkins under her dress, and thus gradually let her rotundity become apparent, rubbed her breasts frequently, in order to develop them, fainted in church, was often ailing and sent for midwives and consulted them concerning her symptoms; physicians were also called upon, and every means taken to make public her happy expectations, so that no one had any suspicion that she was not pregnant. The traces of her monthly sickness were carefully concealed.

"At last, in due time, the young giri fell in labor, which was allowed to advance considerably before the midwife was sent for. In the mean time the bed was arranged in the following manner. A board was taken out of the bottom of the bedstead, and immediately above

this opening a hole was made through the mattress and paillasse large enough to allow the legs of a person to pass through and rest upon the floor. The bed was made in such a manner as to sink down towards the headboard, while it was elevated below the opening in the mattress. The mistress now leaned in a sitting position, with her legs through the opening in the bed, and supported against the headboard, while the servant lay across her lap on a feather bed, in the attitude of labor. Her body was entirely concealed by the bed coverings, which also concealed her mistress up to the neck. The midwife, upon her arrival, found the Baroness, as she supposed, in the throes of labor; she made the necessary examination, promised a speedy deliverance, and gave the usual words of comfort. The lady, however, screamed lustily at every pain, the approach of which she became conscious of by the involuntary contraction of the poor girl's body; while the latter suppressed her cries as much as possible, except when she could mingle them unperceived with those of her mistress. A living male child was soon born, and the after-birth followed it immediately. While the nurse was busy in washing and dressing the child in another room, the girl escaped from the bed into an adjoining chamber. The Baroness, before the return of the midwife, drew her feet up from the opening, covered it over with the bed, and stretching herself out upon it, forbad the midwife, (who was desirous of ascertaining her condition) to touch her, except to wash off the blood with which she had previously soiled her thighs, declaring that she was in so much pain, that she could not endure the slightest touch. The child was baptized, and on the second day put to the breast of the lady. As, however, very naturally, it found nothing there, the midwife was discharged on the pretext that the Baroness' own attendant could now take care of the child, which immediately upon her departure was confided to its own mother. The remainder of the girl's history not being essential here, is omitted. Unexplained circumstances prevented the fraud from succeeding. The authors of the conspiracy fled, leaving the servant-girl sick and in a state of destitution. She died, from the effects of privation and exposure, shortly after having made this confession."* Pp. 242, 243.

Under the head of "General Considerations" on wounds, his view of the proper use and meaning of the term "wound" is thus expressed.

"§ 792. The meaning of the term 'wound,' in popular language, can hardly be misunderstood. Its meaning is, indeed, not precise, nor is the term susceptible of an exact definition. It is applied to most forms of bodily injury caused by external violence, apart from any consideration of the means by which this was inflicted, but, in general, involving the idea of a breach of continuity in the skin. This latter element, however, of the definition, is not included in the surgical meaning of the word, since one class of wounds, called 'contused,' and

* Henke's Zeitschrift, Vol. xliv. d 172.

recognized by all surgical writers, is not attended necessarily with any division of the skin. Indeed, the popular understanding of the expression appears to be more rational than is the professional one, for if a contusion or bruise can properly be called a wound, it is difficult to perceive why a sprain, fracture, or dislocation, is not equally entitled to the name. Yet these latter injuries are never so called by surgical writers. It is difficult, also, in consequence of the apparent vagueness of this term, to know whether burns and scalds can properly be ranked as 'wounds.' The immediate effect of the application of a burning or heated body to the skin, may not be such as to cause more than a redness of the surface, or an elevation of the cuticle into a blister, but the surface of the skin may afterward, by the giving way of the cuticle, be exposed, and it is then termed 'wounded.' Hence, the reader will perceive, that any legal limitations of the meaning of the words, whether based upon popular or professional definitions, are liable to be erroneous, if the intention be really to designate the results of external violence by a name which shall comprise them all. In treating of this subject in its medical aspect alone, we shall make use of the word *wound* as expressive of any form of bodily injury caused by external violence, since it is only by such course that the medico-legal bearings of the subject can be properly considered. Hence we have used the term WOUNDS as a convenient designation for this chapter, entirely irrespective of the possible surgical or legal limitations of the word."

A very wholesome caution is administered in the paragraphs which follow.

"Whatever parts of this examination call for the application of knowledge of which he may not be possessed, as the use of the microscope, or chemical analysis, should be committed to one who is really an 'expert' in these branches. The idea is much too prevalent, and should be corrected, that the practitioners of medicine must necessarily be acquainted with all the appliances and new modes of investigation which modern science has produced; in other words, that every physician is equally competent to undertake the examination of a case involving the question of homicide. It is to this cause chiefly, viz., the disparity in the attainments of one physician as compared with another, and also to the natural division of medical science and practice into numerous departments, some one of which may be cultivated to the exclusion of others—that the 'disagreement of doctors' is really due. Men of equal medical attainments will rarely differ upon an essential point of pathology or practice, but ignorance, or a defective knowledge in medicine, does not differ from that in any other branch of science in being usually associated with presumption and obstinacy. Still there are few practitioners of medicine who are thoroughly prepared to enter upon an examination of all the medical aspects of a case of violent death; familiarity with the means required to carry through such an investigation, can be gained only by special study, for which, to the majority, time is wanting.

Circumstances may, however, impose upon the physician the duty of making an examination for which he does not feel himself fully competent. In remote or interior parts of the country the means for the successful prosecution of a medico-legal inquiry are usually not at hand; who ever may be obliged to undertake an examination under such circumstances, should endeavor to obtain the assistance of a colleague, and should candidly represent to the authorities the necessary imperfection of the examination, and what influence this may have upon the objects of the inquiry." Pp. 540, 541.

Again in regard to suicidal wounds.

"§ 818. That part of the *circumstantial evidence* which requires medical knowledge for its elucidation, is often most curious and important, and as it has to deal with conditions incessantly varying, and is founded upon no familiar principles, nor positive scientific basis, but rather upon loose and badly observed facts, must partake of the same nature, and often appear discordant and improbable. Each medical witness may put together in a different manner, the materials with which he is required to reconstruct the scene immediately preceding death; and a successful result will most certainly reward him, who with the most acute perception unites the largest and most familiar acquaintance with similar facts. In estimating the probabilities in reference to the manner of death, the physician has need of all the aid which a general observation of the workings of the human mind can afford him; his psychological knowledge and his medical experience must here go hand in hand, for it is his task and duty to offer an explanation of the mutual dependence of motives and results, and that, in the same disinterested and merely scientific manner, that would be required by the demonstration of any curious fact in physics." P. 555.

The question of responsibility in the surgical treatment of wounds is well discussed.

"§ 844. Death, indeed, sometimes takes place during or immediately after surgical operations undertaken for the relief of the wounded person. The question of responsibility in this case, belongs to the legal portion of the subject. It may not, however, be out of place to remark that the surgeon can seldom foresee, with confidence, the issue of capital operations, for there are many individual peculiarities and causes beyond his control, which may make it unfavorable. The same may be said of any plan of treatment, whether it involves a serious operation or not. The question may arise, whether the surgical treatment employed was the best that could be devised, and whether, had some other course been pursued, a favorable result might not have been obtained. Or, it may be alleged that the treatment was so unskilful, or the patient so much neglected, as to be the occasion of the fatal termination of the injury. That these facts should be established beyond dispute, it ought to be shown that the treatment was marked by the omission of some-

thing universally recognized as of primary importance. But as every surgeon has some peculiarities in his practice, and as the mode of treatment of bodily injuries, from the progressive nature of the medical art, is various, this omission should be looked for only in those points which betray an ignorance of the fundamental principles of surgery. However much the opinions of competent persons may differ respecting the choice of remedial means, they will generally, we think, be found united upon the principles which should govern their application. Still, occasionally, the plan of treatment may be so singular, although apparently founded upon correct notions of the curative process, as to call for reprobation. Thus, in a case which occurred in Saxony, a surgeon was deprived of the liberty of practising his profession in that country for having attempted to promote bony union between the fragments of a fractured patella, by the novel expedient of firing a pistol between them. Although no permanent injury was done to the patient, who, indeed, a few months after the operation, declared that his leg was nearly as good as the other one, and that he was even able to dance and to walk long distances, yet the medical commission charged with the case very properly considered the operation as likely to prove a dangerous precedent if it were not condemned.*

"§ 845. The difficulty is not so great where the original wound has been trifling, chiefly because its comparatively innocuous character can be clearly shown. Thus, for instance, if the hand have been wounded and one of the arteries divided, compression may be necessary to arrest the hemorrhage. But if a surgeon, with this view, should apply a bandage so firmly, or on the other hand, leave it on so long as to cause mortification of the part, and death ensue in consequence, it is evident that the treatment has not only been unskilful, but that it has really been the cause of death, since the wound of the hand was neither, in itself, mortal, nor would it have produced death in the manner described. But, in severe injuries, in which various complications arise and require the exercise of the greatest skill that learning and experience can give, it cannot be expected that some will not terminate fatally, which, perhaps, under more favorable circumstances, or a better plan of treatment, might have had a fortunate issue. The most humble surgeon may chance to receive the charge of an injury which calls for the enlightened tact and experience of a highly educated man; if his treatment should prove unsuccessful, he should be prepared to show, if required, that his patient had the best care which he was able to afford him, and, if possible, that he consulted with one or more colleagues respecting the treatment. In the language of Judge Woodward, 'The implied contract of a physician or surgeon is not to cure—to restore (e. g.) a fractured limb to its natural perfectness, but to treat the case with diligence and skill. . . . He deals not with insensate matter like the stonemason or bricklayer, who can choose their materials and adust them them according to mathematical lines, but he has a suffering

* Casper's Vierteljahrschrift, 1852, Bd. 1, H. 1.

human being to treat, a nervous system to tranquillize, and a will to regulate and control."* Pp. 569, 570.

The chapter on "Spontaneous Combustion," although brief, is particularly deserving of attention. His belief in the reality of these disputed phenomena is, to some extent, affirmative, as may be seen by the annexed quotation from his opening paragraph.

"Not having adopted a theory with a desire to find those facts only which might be adjusted to it, but desirous of discovering the real extent of our knowledge relative to the phenomenon of what is called spontaneous combustion, we have examined the subject not without some care and earnestness. The result of this investigation has shown us that if there is not such a phenomenon as the actual spontaneous combustion of the human body, there is sufficient evidence to prove, that in some cases it may acquire a præternatural inflammability and that this peculiarity can be recognized by the trifling source of combustion compared with the rapidity and extent of its progress. We do not hesitate also to affirm that a belief in the actual occurrence of the phenomenon referred to may be entertained, without a satisfactory scientific explanation." P. 591.

The subject of medico-legal examinations is introduced with the following admonitions:

"§ 943. The physician who is called upon to make an examination of a person found dead under suspicious circumstances, has devolved upon him a task of no little gravity. He therefore should endeavor to come to it prepared to acquit himself of his duty in such a manner, that he will afterwards not have to regret having imperfectly discharged it. Not only is familiarity with anatomical dissection required, but a far greater carefulness, and a more searching examination than in cases of death from disease, since in these latter, the object of the investigation is into the nature of the morbid cause of death, and the acquisition of greater familiarity with pathological facts. Moreover his attention must be given to many circumstances which, in these, it is not necessary to observe, viz: all those matters which may throw light upon the mode of death, such, for instance as the position of the body in relation to surrounding objects, and the locality in which it is found. The duties of the examiner, and the facts necessary to observe, may be arranged under the following heads:

1st. *Locality.*
2d. *Identity.*
3d. *Indications of violence or unnatural death.*
4th. *Manner of conducting the autopsy.*
5th. *Natural aspect of the organs at different ages.*
6th. *Mode of drawing up Reports."* Pp. 645, 646.

* McCandless *v* McWha, Error to Common Pleas of Beaver County. Am. Jour. Med. Sci. Jan. 1854, p. 273.

These different heads are dwelt upon at length in a very useful series of instructions, of which the summary of the "*Natural aspect of the internal organs,*" is new and particularly desirable as a guide to the post-mortem examiner.

With these few passages from this admirable work we are obliged to close our notice of its valuable contents. We congratulate the publishers on the handsome manner in which they have contributed, in the getting up of the volume, towards the popularity which it so fully deserves, and will doubtless rapidly obtain.

It only remains for us, as the Journal collaborators of Moreton Stillé—his fellow-laborers in other similar paths, and as sorrowing associates who had long since learned to prize his worth,—to say amen with all our hearts to the eminently just and feeling tribute of Mr. Wharton, which he offers in the sadly interesting history of his lamented colleague's last year of study and of its unfortunate and, to us, irreparable end.

"Of the manner in which was performed at least a portion of the task whose history is now given, it may not be unsuitable for the writer who now survives to speak. It was to the preparation of this portion that the last year of Dr. Stillé's short but distinguished professional career was given. It was a year of patient and severe research, marked, to an extent of which the annals of science afford few parallels, by the most self-denying industry, as well as by a rigorous and almost fastidious conscientiousness in the pursuit, not only of truth, but of the most appropriate terms by which that truth could be expressed. And the labor was not one which derived any portion of its severity from the want of prior preparation. Dr. Stillé had not only enjoyed great opportunities for literary and professional culture, but what is rarer, these opportunities were faithfully improved. The liberalizing influences of European culture, as well as the simpler discipline of home instruction, passed not over him in vain. The schools of Europe received from him in the prime of his early manhood the same single and conscientious attention as the schools of Philadelphia in his youth. He found the pleasures of travel and the desultory influences of foreign habits of no more avail in drawing him from a laborious personal attendance on the hospitals—those great repositories of disease which, in the developement of God's wonderful providence, are made the arsenals which supply the weapons by which the maladies that necessitate them are to be combated—than he found the more primitive habits and more limited associations of his native city. The great continental tongues, in their scientific as well as their popular relations, were mastered by him to a completeness of which, among persons of his age, we have rare examples. Few men,

even among the most mature, have gone to the grave so richly fraught with the literature of a profession, which to him was a philosophy as well as an art. And few, at so early age, have gone to the grave with faculties under more complete moral discipline. To one of his most remarkable qualities,—that delicate and yet modest firmness of perception which, unwarped on the one side by pride in his own opinion, or on the other by undue deference to that of others, enabled him, after the most difficult and subtile research, not only to reach but to express the truth—no one has had better cause to testify than the present writer. The work which these lines now close was one which brought both of those who engaged in it into the most intimate and affectionate personal intercourse for many months; and the one who survives can now scarcely look back upon the preparation of a single page without having additional cause to remember and record those high mental qualities and culture, whose value in the present case was only increased by the gentleness, the refinement and the fine sense of personal honor with which they were associated.

. . . . . .

"Perhaps, indeed, even in a profession whose history has been so marked by acts of zeal and disinterestedness, when we take into consideration the fact that Dr. Stillé was impelled by no other motive than that of professional love and enterprise in the severe course of study and self-sacrifice in which he was engaged, there will be found few cases where these qualities have been so eminently exhibited as the present. Possessed of an ample fortune, he was one of those uncommon instances in which the most arduous and protracted courses of preliminary trial are gone through with under the calm and equal effect of a will which is impelled neither by necessity nor the desire of present applause, but by the faith in a distant future, in which the result will be none the less precious because it is the longer delayed.

"But this future was one to which Dr. Stillé—and to the great loss of popular as well as of medical science—was only in part to arrive. Early in 1855, he received the appointment of Lecturer on the Practice of Medicine in the Philadelphia Association for Medical Instruction, and at the end of June closed the first portion of a course of lectures of which it is not too much to say that they were received with unmixed satisfaction by the class to whom they were addressed, and the colleagues with whom he was associated. In the first week of July he sent from his office the last of the manuscript of that portion of the following pages which fell under his charge, and almost immediately afterwards was stricken down by a disease which found him with strength impaired by the exhausting studies of the preceding winter. On August 20, 1855, he died at Saratoga, almost at the moment when the press was issuing the last sheets of a work which contains so much worthy of being erected as a monument in which his professional brethren will recognize the impress of his high intellectual gifts and culture." Pp. iv. v. vi. of Preface.

Principles of Human Physiology, with their chief applications to Psychology, Pathology, Therapeutics, Hygiene and Forensic Medicine. By WILLIAM B. CARPENTER, M. D., F. R. S., F. G. S., Examiner in Physiology and Comparative Anatomy in the University of London, &c., &c. *A new American, from the last London Edition, with two hundred and sixty-one illustrations. Edited, with additions,* by FRANCIS GURNEY SMITH, M. D., Professor of the Institutes of Medicine in the Medical Department of Pennsylvania College, &c. Philadelphia: Blanchard & Lea. 1855.

This is the sixth large edition (as we are informed) of Dr. Carpenter's Principles of Human Physiology, a sufficient proof, were one wanting, of its acceptableness to the student and practitioner. In many of our schools it has become the text book on the "Institutes," taking the place of others that for a long time held undisputed sway. A modification has been introduced into the present volume, however, which we fear may, to a certain extent, mar its usefulness as a companion to the student, though it will probably enhance its value in the eyes of the practitioner. This consists in the carrying out of a plan long entertained by the author, of making three distinct treatises of the Human, Comparative and General Physiologies, transferring to each of these volumes whatever was found in the other relative to the special departments, thus making three "independent but mutually connected treatises on the three great departments into which modern physiology naturally divides itself." In accordance with this plan, the chapters on animal chemistry and on the structure and actions of the animal tissues, which occupied about 240 pages of the last edition, have been entirely omitted in the present, and the student who desires information on this important subject must look elsewhere for it, if his means permit of his obtaining the more expensive works in which it may be found, or—go without it. This omission, we feel constrained to say, we consider a capital mistake, and one which we think must eventually limit the circulation of the "Human Physiology" to the more advanced student and practitioner, and bring his "Elements" more prominently forward as a substitute in the hands of those engaged in attendance upon lectures.

With this exception, the present edition cannot fail to sustain the enviable reputation obtained by previous ones. Dr. Carpen-

ter's well known zeal and industry are fully shown in the care evinced in the remodelling of some of the chapters, and the additions of new material by which the sixth edition is fully brought up to the times. Everything appertaining to the science which has appeared since the last edition, has been subjected to his reducing crucible, and a complete digest is afforded to those whose opportunities of research are more restricted. The views of the author are too well known to our readers to make any analysis necessary; suffice it to say, that where additional light has been thrown upon any subject, making a change of opinion necessary, the author has not hesitated to express it; a frankness calculated to inspire confidence in his readers and to insure a faithful reflection of the present state of physiological science.

The editor's notes are both full and numerous. We have been especially interested in his remarks upon the origin and destination of fibrin in the animal economy, and with the theory of the cause of the heart's action, and commend them to the reader's attention. Among the additions we observe Prof. Dalton's excellent article upon the Gastric Juice, published in the American Journal of Medical Sciences; Mr. Joseph Jones' paper on Endosmose, read before the Philadelphia Academy of Natural Sciences, and Professor Jackson's philosophical, satisfactory, and we believe entirely novel explanation of the functions of the semi-circular canals of the ear, besides a number of other valuable American and foreign contributions to the science, which have passed unnoticed by the author. Unusual judgment is shown, in our opinion, in these selection and additions, and we are confident that those who read them will agree with us that they materially enhance the value of the work.

The publishers deserve great credit for the handsome style in which the volume is printed and illustrated.

The Diagnosis of Surgical Cancer. (*The Liston Prize Essay for* 1854.) By JOHN ZACHARIAH LAURENCE, Surgeon to the Northern Dispensaries; late House Surgeon to University College Hospital. London: John Churchill. 1855. pp. 77, in two chapters, with two plates.

We have read this short monograph with considerable interest and have been so much pleased with its mode of treating its im-

portant and difficult subject, that we regret our inability to give it more than a cursory notice.

It is modestly offered "rather as a rough sketch of the Pathology and Anatomy of Cancer (which may one day serve as a basis to future investigation), than as laying any pretensions to the character of a finished treatise." Regarding it in such a light, it certainly does great credit to the scientific and practical training of its author; and at the same time encourages us to look to the same source for a much more extended and conclusive examination of the whole question, to which it already forms so excellent a ground work.

Justly considering the diagnosis of a case to be more or less concerned in the consideration of all its points, he briefly discusses the diagnostic value of these, severally, according to the following scheme:

"I. ÆTIOLOGY. *a. Predisposing causes.* 1. Hereditary; 2. Personal; sex, age, previous health. *b. Exciting Causes.* II. SYMPTOMATOLOGY. *a. Special Previous History.* 1. Progress of growth; 2. Patient's general health during that progress. *b. Condition of patient when first brought under observation.* 1. Local Phenomena; (consistence and aspect of growth, pain, hemorrhage, discharge, &c.); 2. General Phenomena. (Cachexia, wasting, &c.) *c. Anatomy of growth.* 1. Obvious Characters; 2. Microscopic Characters."

In this discussion he avails himself, in the first place, of the labors of Lebert, Paget, Walshe, Velpeau and others, together with some of the older observers, as Wardrop and Hey; and in the second place, of his own individual observations.

The clinical aspect of the subject, including causes and symptoms, occupies the first of the two chapters. It is treated in a practical manner, and although on account of its brevity it is necessarily more suggestive than conclusive,—more likely to unsettle than determine mooted questions—it is valuable in this very way and furnishes many useful hints. The same thing, to some extent, may be said of the remarks upon the anatomical elements of diagnosis, which are the subject of inquiry in the second chapter. We have only room to quote the author's concluding estimate of the microscopic test as a means of diagnosis.

"1. That in the greater number of cases of cancerous tumors the so-called cancer-cell will be found.

2. That this form of cell is occasionally seen in growths manifestly innocent.

3. That, *vice versâ* (what is, however, less frequent), tumors anatomically innocent prove clinically malignant—that 'the cancer-cell is not the *sine quâ non* character of cancer.'

4. That the inferences drawn from the microscopic examination are not to be deduced from a few isolated cells that may have happened to strike the eye, but rather from the characters of all the cells, and of the field of view generally.

5. That the results afforded by the microscope must take an important but not an exclusive and overbalancing position in the series of data, which are to serve us as the premises for our conclusion."

The Book of Prescriptions: containing 2900 *Prescriptions, collected from the practice of the most eminent physicians and surgeons, English and foreign. Comprising also a Compendious History of the Materia Medica of all countries, alphabetically arranged, &c.* By HENRY BEASLEY. Philadelphia: Lindsay & Blakiston.

Mr. Beasley's book belongs to a class of works, which in their proper place and as remembrancers are exceedingly useful. There are few physicians who have not their favorite prescriptions for particular diseases or stages of disease; it is impossible it should be otherwise where the practice is large; it is well for all, therefore, to be provided with a work like the present, in which can be found under the head of each remedy the various methods of prescribing it, adopted by practitioners of authority in each instance. Dr. Beasley is evidently an industrious, well-informed man, in every way qualified to supply this want, and we take much pleasure in recommending his work to the profession.

"It will readily be imagined," says the author, "that the prescriptions are not all of equal merit, and the reader will find several opportunities of comparison, by which a considerable variety of opinion among medical practitioners, particularly in the matter of doses, has been forcibly illustrated. When it has been necessary to point out the mean between extremes of this nature, the editor has been assisted in his task by competent medical advice, and he hopes that a correct judgment has been thereby arrived at."

The following is a sample of the work:

ACIDUM HYDROCYANICUM. *Hydrocyanic or Prussic Acid.*

Hydrocyanic acid is a direct sedative, and so highly poisonous, that a single grain of the pure acid is sufficient to destroy life. The diluted acid, in medicinal doses, allays irritation, reduces the pulse, and lowers

the sensibility of the nervous system. It is used to quiet irritable and spasmodic cough, to allay vomiting, and nervous palpitations, and to relieve pain and quiet the system in neuralgic, rheumatic, and other painful affections. *Externally*, it is used in lotions, to allay itching in some cutaneous diseases. *Inhaled*, it has been tried in some affections of the lungs.

The usual dose of acidum hydrocyanicum dilutum, L. is from 3 to 5 minims. That of the new DUB. Ph. is, perhaps, intended to be of the same strength, but its percentage of real acid is not stated. The process yields a product which is variable, but usually stronger than that of L. Acidum hydrocyanicum, E., is stronger than L., 3 minims of the former being equal to about 5 of the other. *Acidum Hydrocyanicum (Scheelii)* is often met with in prescriptions; but there is no standard strength for it, and it possesses no advantage over the pharmacopœia preparation. The average strength, as procured from various manufactures, seems to be about twice that of L. *Acidum Hydrocyanicum* of the United States and Prussian Pharmacopœias contains, like that of L., 2 per centum of real acid.

Potassii Cyanidum has the same properties and uses as hydrocyanic acid. Dose from one-eighth to one-fourth of a grain. The latter dose is equal to 5 minims of diluted Hydrocyanic acid, L.

℞ Acidi Hydrocyanici dil. ♏j.
Aquæ destillatæ, f ℥vij.
Syrupi Aurantii, f ℥j.

M. fiat haustus quaque secunda hora sumendus donec evanescent symptomata.

In Gastric Irritability, Nervous Palpitation, &c.—DR. NELIGAN.

℞ Acidi Hydrocyan. dil. ♏xij.
Aquæ destillatæ, f ℥j.
Syrupi Papaveris, f ℥iij.
Misce: capitat cochl. amplum secunda quaque horâ.

In Consumptive Cough, &c.—DR. GRANVILLE.

℞ Potassæ Bicarbonatis, gr. xv.
Cocci Cacti, gr. viij.
Aquæ destillatæ, f ℥vj. Tere simul, cola, et adde.
Acidi Hydrocyanici dil. ♏x. Misce.

A tablespoonful to be taken when the cough is troublesome.

In Hooping Cough of Children.—DR. GRANVILLE.

℞ Misturæ Amygdalæ, f ℥vss.
Acidi Hydrocyan. dil. f ℥ss.
Tincturæ Opii, f ℥ss.
Tinct. Lavand. comp. f ℥ij.

Misce: fiat mistura cujus sumantur cochl. ij. larga secundis vel tertiis horis.

In Gastrodynia with Spasms.—DR. COPLAND.

℞ Acidi Hydrocyanici, guttas v.
Aquæ Calidæ, q. s.

Inhale the vapor by means of a suitable apparatus three times a day,

lying down an hour after each; very gradually increase the dose to ten drops. After four or six weeks, give steel and quinine.

In Hypertrophy of the Heart.—Dr. T. G. Hare.

A Dictionary of Terms used in Medicine and the Collateral Sciences. By Richard D. Hoblyn, A. M. Oxon. *Revised, with numerous additions,* by Isaac Hays, M. D., &c. Philadelphia: Blanchard & Lea. 1855.

This excellent little dictionary, now in its sixth edition abroad, has been considerably enlarged and improved by the introduction into it, by the American editor, of a large amount of new matter, in which the student will find, besides many terms recently added to science, the names of our native medicinal plants, the formulæ for the officinal preparations, &c., &c.

The preface states that "the editor has availed himself of very many recent sources of information in preparing his additions, among which he would especially mention the *Expository Lexicon*, by Dr. R. G. Mayne; *Medical Botany*, by the late Dr. R. E. Griffith; the recent works of Carpenter, Paget, Owen, and Jones and Sieveking; and the admirable *United States Dispensatory* of Professors Wood and Bache."

An additional inducement to the American student to purchase the work, is, that it has been made to conform with the latest edition of the United States Pharmacopœia.

We warmly commend it to our readers.

The Transactions of the American Medical Association, instituted 1847. Vol. VIII. Philadelphia: Printed for the Association by T. K. and P. G. Collins. 1855.

Our limits prevent us from doing more than acknowledging the receipt of the above volume. We shall take another opportunity of noting its contents.

THE MEDICAL EXAMINER.

PHILADELPHIA, DECEMBER, 1855.

MEDICAL NEWS.

A Consumption Hospital, the first of the kind in the United States, has been recently organized in New York, by the election of Dr. J. H. Griscom, as President, and Dr. A. H. Jones, as Secretary. Buildings will be erected as soon as the necessary funds are raised.

We regret to notice the death of Professor Johnston, of Durham, a well known Chemist and writer on scientific subjects.

Professor Allman, of Dublin, has been elected to the Chair of Natural History, in the Univerity of Edinburgh, vacated by the death of Prof. Forbes. Dr. Laycock, of York, a well known and distinguished writer, has succeeded, after a vigorous canvass, in obtaining the Chair of the Practice of Physic in the same Institution. His strongest competitors were Professor Bennett and Dr. Wood, of Edinburgh. Professor Alison, the previous incumbent, whom ill-health caused to retire, has been appointed Emeritus Professor.

British Mortality in England and the Crimea.—If all the deaths of British soldiers in the Crimea during the last three months were added to the deaths in England, the sum would be less by some twenty thousand than the deaths registered in England during the three summer months of 1854. In fact, it results from the Report of the Registrar General, for the Quarter ending September, 1855, that the summer months of the present year have been unusually favorable as regards health, notwithstanding the undoubted disadvantage under which the poor have suffered in respect to the price of provisions. The causes assigned for the improvement in the public health, by the District Registrars, are various, but they are quite in unison in respect to the inestimable advantages accruing from sanitary improvements. The deaths in the summer quarters of the five years 1851–55 were as follows:—

1851.	1852.	1853.	1854.	1855.
91,499	100,382	92,201	113,939	87,934

It is a curious circumstance, in reference to the means of livelihood of the people, that while in the interval of two years the price of wheat has risen 48 per cent., and that of beef 15 per cent., there has been a fall of 37 per cent. in the price of potatoes, an article of large consumption among all classes, and which has operated as a set-off against the

high price of other articles; thus, perhaps, partly accounting for the fact that the public health is better than in the summer of 1853. In reference to the Meteorology of the quarter, Mr. Glaisher reports that the departures from the average values of the temperature have in no cases been large during the quarter, and that other subjects of investigation have been about their average. Such being the facts of the case, it remains only that, eschewing the insignificant few who, for peculiar reasons, are *sans* olfactories, we steadily pursue the work of sanitary reform, having confidence in the aphorism, "*Aide-toi, et le Ciel t'aidera.*" —*London Medical Times.*

Card of the Committee on Prize Essays of The American Medical Association.—At a meeting of the American Medical Association, held in Philadelphia, May, 1855, the undersigned were appointed a Committee to receive voluntary communications on Medical subjects, and to award prizes in accordance with the regulations of that body.

Each communication intended to compete for a prize, must be addressed to the Chairman of the Committee, at Ann Arbor, Michigan, before March 20th, 1856, and must be accompanied by a sealed packet, containing the name of the author, and marked exteriorly by a sentence or motto, corresponding with one upon the essay; which packet will not be opened unless the essay belonging to it is successful in obtaining a prize. Unsuccessful papers will be returned on application, after the adjournment of the Meeting of the Association, in Detroit, in May next.

A. B. PALMER, M. D. (Chairman), SAMUEL DENTON, M. D.,
A. R. TERRY, M. D., A. SAGER, M. D., S. H. DOUGLASS, M. D.,
C. L. FORD, M. D., E. ANDREWS, M.D.

We have been desired to copy the following notice. We sincerely hope that its object, which all must allow to be a very laudable one, will be crowned with success.

Philadelphia, November, 1855.

Dear Doctor:—At the last meeting of the Pennsylvania State Medical Society the following Resolution was adopted:—

"*Resolved*, That the Corresponding Secretary be, and he is hereby instructed to address a circular to the prominent Physicians in those portions of the State in which no County Medical Societies exist, urging upon them the importance of organizing such Societies without delay."

In this age, the influence of associated action is invoked by every class of the community which has interests to protect, or objects to accomplish; it cannot, therefore, be regarded as singular, that Physicians should seek its aid to protect the interests of their Profession, which, in this State, the law leaves entirely unguarded.

We do not ask your assistance to obtain Legislative Protection, but we earnestly urge you to assist us in protecting ourselves: organize a County Medical Society without delay, enroll the names of the educated, moral, and regular Physicians, and exclude from the brotherhood the ignorant, the irregular, and the unprincipled; then will your brethren at a distance know to whom to apply for advice and assistance, and will co-operate with you in all measures calculated to extend the usefulness of our high calling, and to elevate, dignify and adorn it. Bring

to your County Society the results of your observations and experience in Medicine and its kindred sciences; collate them and send them to the State Society for common improvement, and receive in return the observations and experience of Physicians in other parts of the State; thus will all be benefitted, and much valuable information be preserved and circulated.

The scandal of our profession is, and always has been, that Physicians not only differ in their treatment of disease, but that they do not live together in social harmony. The formation of a County Society leads to more frequent professional and social intercourse, and thus elevates the character of the profession by discouraging the spirit of ungenerous rivalry, reconciling enemies, and healing those differences which arise from mi-representation or fancied injury.

Our annual reunions are, moreover, seasons of pleasure as well as profit, for in them, warm-hearted, accomplished, and distinguished Physicians, from all parts of the State, meet in social as well as scientific fellowship.

The next meeting of the State Society will be held in Philadelphia, on the last Wednesday in May next, when we will be happy to receive you as a delegate or individual member from your County, and will endeavor to make your sojourn in the Medical metropolis of the country as pleasant as possible.

Yours, very respectfully, THOS. H. YARDLEY,
Corresponding Secretary of State Medical Society.

NOTE.—The objects of the Pennsylvania State Medical Society, as declared by the Constitution, are "the elevation of professional character; the protection of the interests of its members; the extension of the bounds of medical science; and the promotion of all measures adapted to the relief of suffering, and to improve the health and protect the lives of the community."

The members consist of ASSOCIATES and DELEGATES; every member of a properly organized County Society is an associate; the delegates are elected by the County Societies in the proportion of one to every five of its members; but every County Society, however small, is entitled to one delegate.

The Delegates meet annually, to transact the business of the Society. The first meeting was held in the City of Reading, in 1849, at which only ten County Societies were represented; now twenty-five Counties have formed Societies, and we hope at the next meeting to have delegates from every County in the State.

A volume of the *Transactions* is published annually, containing the names of the Officers and Members of the County Societies, their reports, and much other valuable information. A few volumes of these *Transactions* remain on hand, and will be forwarded to the address of any Physician who is willing to take the first step for the formation of a County Society; or any other information on the subject will be given on application to

THOS. H. YARDLEY,
No. 381 *Arch Street, Philadelphia.*

RECORD OF MEDICAL SCIENCE.

Notes on the Application of Statistics, to Questions in Medical Science, particularly as to the External Causes of Diseases. By W. P. ALISON, M.D., Edin., D.C.L. Oxon., Emeritus Professor of Practice of Medicine, Edinburgh.*

It has so frequently and so plausibly been urged, against all the inquiries and studies which are termed Statistics, that the form of such reasoning may be applied to the support of almost any proposition, that

* Read to the Statistical Section of the British Association at Glasgow, Sept. 1855 (by Sir Archibald Alison.)

it becomes an object of very considerable importance,—in the view of any one who is truly convinced of the importance and frequent practical application of such inquiries,—to point out the circumstances of any question, or departments of any science, in which this kind of information is truly requisite, and the conditions under which it may be trusted. This is especially true of the science of Medicine, because there is one great department of that science,—that which we term Etiology, or the doctrine of the external causes of diseases, in which our knowledge is acquired almost entirely in this way. It is in very few cases only, that our knowledge of the essential or intimate nature, either of diseases or of the powers in Nature which excite them, enables us to form any anticipation of the effects of those powers; and it is simply by *empirical* observation,—facts observed and recorded, and the frequency of their recurrence noted, although not explained, *i. e.*, it is by the mere *force of numbers*, or by *statistics*, whether stated exactly in that form or not, —that our information on that subject, and practical rules for the prevention or treatment of diseases, founded on that information, are acquired.

I have elsewhere stated,* what seem to me to be sufficient reasons, for the application of Statistics to inquiries of this kind being more frequently and obviously required, than in any of those which are made, either into the nature of diseased actions (or of vital actions in general,) or the power and mode of action of remedies. The questions which we propose to ourselves in Etiology are truly *simpler* than either in Pathology or Therapeutics,—they involve little or no exercise of judgment, simply the observing and recording of facts, according to directions which are made known, and the sufficiency of which can easily, at any subsequent time, be estimated;—the sources of fallacy connected with them are *less numerous;* and, more especially, we can usually remark this, as to the observations by which we can fix on those antecedents of a disease, to which we ascribe the power of producing it,—that the *positive observation*, of the alleged effect following the application of the cause, is supported by a large body of *negative observations*, often not stated in words, but truly essential to the validity of the inference; and, therefore, this class of observations is often more truly and efficiently statistical,—the number of individual cases really contributing to the result obtained, is very much greater, and the evidence afforded more decisive, than it can easily be made in the other departments of medical inquiries; sometimes, therefore, greater than those who have accustomed themselves only to physiological or pathological inquiries, or to watching the effects of remedies, can easily perceive.

The importance of this observation will not be denied, when we remember that this is the department of Medical science which is truly of the greatest practical importance. The knowledge of the external causes of diseases, is that which leads most directly to their prevention; and to the preservation of those lives especially, from which the greatest amount

* British and Foreign Review, etc. January and April 1854.

of labor of all kinds may be obtained, and which are, therefore, generally regarded as most valuable to a state.

In the course of the present century, improvements have been made in Medicine, which will bear comparison in their practically beneficial tendency, with those which have made this age and this quarter of the globe so illustrious, as regards the applications of any other Sciences to practical purposes. These have been almost exclusively in this department of medicine, and may be truly said to rest, as yet, almost exclusively on Statistics; anticipating, probably by several ages, any information within the power of the human race, as to the intimate nature of the phenomena which are thus recorded. It is simply by the force of numbers, attesting the simple fact of a disease, easily recognised, showing itself in many persons within narrow limits of time and space, and there only, that we have learnt how the poison of Small-pox may be diffused; and that it may be disarmed of its power over the human body, by being first taken into, and modified by its passage through, the body of the cow, and then applied, in almost infinitesimally small quantity, to the human body itself, and there exciting a certain specific inflammatory process, absolutely devoid of danger, and incapable of communication through the medium of the air, and, what is still more inconsistent with the knowledge we have of other changes in nature, that this modifying and preserving effect on the human body, produced in infancy, continues equally perfect, in a great majority of cases, after 60, 70, or 80 years, if life shall continue so long,—*i. e.*, after we are certain that the body on which it has been produced, has been repeatedly *worn down and built up again*, so that the poison introduced into the structure in advanced life, can no more be said to come in contact with living matter which has gone through the process of vaccination in infancy, than, according to the paradox of an ancient logician, a man can be said to have bathed twice in the same water, because he has bathed twice in the same river.

So also it has been simply empirical observation, and, therefore, by reference to statistics, that we have acquired within these few years, information touching the extension of another epidemic, less frequent, indeed, but attended with peculiar interest, and often with extreme fatality, the Puerperal fever, which enables us, with almost absolute certainty, to predict that its propagation, after the manner of an epidemic, may hereafter *always be prevented.* For, I believe, I may safely assert, that the statistical observations of Dr. Semmelweiss, in the great Lying-in-Hospital at Vienna, where upwards of 6000 births take place in a year,—being in exact accordance with what has been seen in this country, have unequivocally shown:

1. That this disease is, in fact, a case of the Diffuse inflammation or Erysipelas, attended with the same peculiarities in the extension of the inflammation, and in the nature of the effusion, and the same variation as to the nature of the accompanying fever in different epidemics.

2. That one immediate exciting cause of this form of inflammation, which may always be suspected when it prevails epidemically, is the *cadaveric* poison,—often evolved, during the decomposition of the human

body, from whatever cause death may have taken place ; but during one stage of that decomposition only, viz., that early stage during which the *post mortem* inspections for the investigation of the cause of death are most frequently made.

3. That when this form of inflammation assumes the form of epidemic puerperal fever, the mode of its transmission from one patient to another, is by accoucheurs or nurses, themselves in good health, but to whose persons or clothes minute portions either of the *effluvia from others* already affected, or of this *cadaveric poison*, have become attached; and

4. That when the obvious precautions are taken, to prevent any persons to whom, in either of these ways, the poison can have thus adhered, from acting as accoucheurs or nurses until effectually purified, no "epidemic extension" of puerperal fever is seen.

It is seldom, no doubt, that this last proposition can be submitted to a searching scrutiny, as in the year 1846, at the Vienna Hospital, under Dr. Semmelweiss, when the number of deaths after these precautions were adopted, was diminished by no less than 400 in that hospital alone, and in a single year; but when it is added, that each of the propositions above stated is quite in accordance with observations made and recorded (although the bond of connection among them had unfortunately not been duly observed) in this country, whenever puerperal fever has been epidemic, whether on a large or small scale,—so that it would be easy to collect statistical evidence of the same kind from every such epidemic, wherever any record of the facts has been left, to establish each of these propositions,—the force of this statistical evidence becomes such as to justify the sanguine anticipation above announced.

Our information as to the mode of extension of the malignant Cholera, is, unfortunately, as yet less certain or precise; yet I think we may say it is so far advanced, that we may entertain a confident *hope* of its being very soon such as to disarm any epidemic cholera, in this climate, at least, of all its terrors.

I allude to that subject here, chiefly because I think it affords an example worthy of notice, of what I have already stated, as to the frequent misapprehension of the strength of evidence, simply empirical, or founded on statistics, which may often be obtained in inquiries of this kind.

I mentioned formerly in a paper on this subject, which I had the honour of laying before this Association at Birmingham, a *single case* which I saw in the year 1832, and which I have always maintained to have been sufficient, for any one who duly attended to the statistical evidence it afforded, to establish the proposition, that the disease is capable of propagation in this climate, by the intercourse of the sick with the healthy, without pledging us to any opinion as to the mode of communication, or as to the existence of other modes. Now that it is generally admitted, that the assertion then made was correct, the question for discussion now being (as stated by the Editor of the *British and Foreign Medical Review*), "*not* whether cholera is contagious or not, but *how often* it spreads *by the agency of human bodies* (*i. e.*, by *contagion*), and how often without that agency," (Journal for January 1854, p. 298.)

I think myself justified in drawing attention to the grounds on which it was made; and which, I still think, amply sufficient to establish that mode of communication, without, of course, excluding others.

The reason of this strong expression of opinion was, that the case furnished a remarkable example of the evidence which, according to what was formerly stated, a single *positive* fact may afford,—quite of the nature of the *instantia crucis*, when supported by a large body of *negative* observations. The anxious expectation of the disease in the town, in 1832, the careful division of the town into districts, and appointing of stations and medical men to each, and the number, zeal, and intelligence of these observers, were known to be such as to justify our saying, that this was the very *first* case of the disease which ever originated in Edinburgh or Leith, in a person who had not left the town; and that for a time it was *an isolated* case, no other appearing for ten days, in a population of above 140,000 persons, many of whom were in circumstances, in all other respects at least, equally favorable to the appearance of Cholera, as this woman was;—as the subsequent appearance of the disease in many of them, during nearly a year that the disease afterwards existed in Edinburgh, proved. If the poison producing this "*nova pestis*," had no contagious property, this woman was not more exposed to it than any other of this large body of people; but if it had that property—in whatever other way it might be communicated,—she was undeniably and peculiarly exposed, as she was engaged in nursing her son, in a close confined room, who was ill of the symptoms of cholera, during the whole of the day preceding that in which she sickened and died; and he had passed the next preceding night in a house in Musselburgh, where patients in the malignant disease then were, and had been for weeks previously. Here, then, was a fair *instantia crucis*, to determine whether the transmission of the disease, from place to place, known to be at that time a frequent event, was or was not dependent on intercourse of healthy persons, with those previously sick of it; and the affection of this one person, who had that intercourse, contrasted with the non-appearance of the disease in the 140,000 was, as I maintain, *decisive* evidence.

Many cases, equally decisive, have since occurred, of which I have been informed, on the *first* appearance of a disease *previously unknown* in a town or district, carefully observed;—*e. g.*, at Arbroath in 1853, where there was clear proof that the two first persons, of 15,000 inhabitants of that town, who took Cholera, "had just returned from Dundee, where they had visited persons ill of cholera;" their affection, therefore proves nothing as to the efficacy of intercourse with the sick, in exciting the disease, rather than merely visiting a particular locality;— but the next *six* who took it in Arbroath,—the *first six* inhabitants of that town who took cholera without leaving the town, had repeated and close intercourse with those already affected before they took it; and the inference as to the contagious property is drawn, not from their taking the disease, nor even from their taking it at that time, and in rapid succession, but from their being *the only inhabitants* of Arbroath who took it, during the first week of its existence in that town;—*i. e.*, the positive evidence of the *six* who were known to be exposed to that cause, is supported by

the *negative* evidence of the *fifteen thousand* who could not be shown to be, and the immense majority of whom certainly were not, so exposed.

I have great hopes that we shall soon have statistical evidence to establish a proposition which no other evidence known to us could justify our admitting, but which, if established, would not only reconcile most of the conflicting statements on this subject, but serve as a guide to almost complete security from any epidemics of this disease in future; viz., that the poison of cholera, *like other known animal poisons*, is developed during the decomposition of the animal matter, the appearance of which is most characteristic of the disease,—that which constitutes the *"rice water stools,"*—but only in a *particular stage* of that decomposition, not immediately after its formation in, or discharge from, the body; and again, not after the decomposition has gone to a certain length. All this can be proved only by Statistics, but is quite susceptible of proof in that way. The last proposition above stated—that no poisonons property is attached to any part of a body dead of cholera, after a certain stage of decomposition has been passed, seems nearly ascertained from the number of instances in which dissecting-rooms have been supplied with bodies of persons dead of cholera, for many weeks together, without any of the students attending them being affected. That the peculiar matter of the cholera evacuations, during its decomposition, perhaps especially in dry air, has a peculiarly poisonous quality, was, I believe, first suspected by Liebig, and partly by the analogy of the poison already mentioned, causing erysipelatous inflammation and puerperal fever,—and also of the *sausage poison*, repeatedly observed on a large scale in Germany, which is developed during a certain stage of the decomposition of the animal matter of those sausages, and disappears when their putrefaction has so far advanced. That the cholera can be communicated to animals by inoculation with this matter, of the peculiar rice water stools, has been sufficiently proved by the experiments of Dr. Lindsay in this country; and if a few more experiments shall give results similar to those of M. Thiersch, at Munich, I think we may assert that the proposition above stated, is *statistically* proved. "Dr. Thiersch collected the intestinal contents, or the evacuations, of cholera patients, and let them decompose under the influence of air and heat. From day to day he dipped into this matter pieces of filtering paper, which he dried, for subsequent experiments on white mice. Two of these animals at a time were exposed to infection for four days, by having a square inch of the filtering paper, thus prepared, moistened with water and mixed with their food. Each mouse took thus 1-2000th gr. daily. The results were as follows:—The preparations from the matter during the first day of decomposition were *innocuous*. To this succeeded a period of from six to nine days, during which decomposition went on, and preparations from the matter in this second period of decomposition, caused disease in 30 out of 34 animals, and death in 12 of the 34. The symptoms were peculiar and characteristic. The hair fell off, the ears dropped, there was languor, then discharge from the bowels, first of white, then watery matter, the urine lost its smell, then was suppressed, the appetite became *depraved*, so that the animals would fill their stomachs with wool; there was no apparent sickness, but such tonic muscular contractions that they seemed

dead some time before death. On dissection, accumulation of blood in the vessels of the small intestines was invariably found, their contents *watery*, with *abundant epithelial flakes* (just similar to those found in persons dead of cholera); the cortical substance of the kidneys passing into fatty degeneration, the bladder empty, the blood and the contents of the intestines answering to the test of ready mixture with amygdalin, as in the cholera of man,—which will not appear in the healthy animal. To this a third period of decomposition of the matter under trial succeeded, in which these poisonous effects were very slight, or not observed at all."—(See *Medical Times* and *Gazette*, Nov. 25, 1854.)

We have already a statement by Dr. Budd, of statistical observations made on villages in England, where the entrance of Cholera appeared to be prevented by such expedients as these observations immediately suggest, for receiving the rice water evacuations of cholera patients on linen or cotton, and burying or burning, or otherwise effectually destroying their substance, during that period, thus indicated, after they have been passed, and before they have entered on the morbific decomposition; and we have sufficient statistical evidence of the importance of another measure,—which was first adopted, I believe, at Edinburgh in 1832, and has since been recommended by the Board of Health in London, and adopted in different places, although not so generally or satisfactorily as could be wished,—founded on the merely empirical statements I have made as to the communication of the disease from the sick to the healthy, and its apparent adhesion to particular, often very limited, localities, viz.,—the establishment of *houses of refuge* for the reception of all inhabitants of houses or rooms which might become infected with cholera in any town; not themselves affected, nor required for the care of the first cases that might occur. Here such persons might be lodged in pure air, regularly fed, preserved from cold and from other (frequently concurrent) exciting causes of the disease, and treated with due attention to cleanliness, immediately on any symptoms of cholera showing themselves. The London Board of Health report, that they had information of 1691 persons taken into these houses of refuge, from rooms where there were patients in cholera, and of these, only 33 became affected with cholera, and only 10 died. In cases of which I was myself informed, in Edinburgh, at Glasgow, and at Oxford, during different epidemics, 1010 persons were admitted from sick rooms into such houses of refuge, of whom 40 took the disease, and 15 died; whereas the experience of Dr. Hamilton of Falkirk, of 251 cases of cholera appearing in 86 houses, where no such means of separation existed, gives only a fair idea of the extent to which successions of cases will often be observed, in confined air and dirty districts, possessing no such resource.

I am happy to say, that so far back as November last, having written to Dr. A. Smith, at the head of the medical department of the army, on the subject of the decomposition of the rice water stools, as the probable cause of the progagation of cholera, and of districts becoming *tainted* with the poison of that disease,—I was informed by him that he had directed the attention of the medical officers in the Crimea to the facts now stated, so that if the disease shall appear in a malignant form in that

army, we may hope for at least accurate and truly statistical information as to the reality of that opinion.

Again, as to the Yellow Fever, so frequently becoming epidemic in the hot climates, although we cannot boast of having acquired information either as to the nature of its cause, the essential character of the morbid change it produces, or the power of any remedy over it,—yet by simply empirical observations, *i. e.*, by Statistics, we have information to the following effect, as stated in reports in Germany and France, on inquiries conducted by order of those governments, on a large scale, and considered by committees containing the names of Humboldt and Dupuytren, that it is a disease *endemic*, almost exclusively, "in districts nearly on the level of the sea, never appearing beyond 48° of north latitude, nor without a previous temperature of 72°,—only in certain circumstances propagated by contagion," but when epidemic, always *confined strictly to certain localities;* so that the practical rule of immediately evacuating, *i. e.*, removing *all* the inhabitants of places where it is declared to exist, and has formerly prevailed, is *incontestable*, and "of such proved utility, as will always justify its rigorous execution."

Without dwelling farther on the effects of Malaria, in this or any other climate, which we can only expect to be satisfactorily explained when pathology shall be considerably more advanced than at present, I may merely add, that such collections of facts have been made, and are now frequently repeated, as to the places and circumstances in which it arises from the earth, and the laws according to which it extends and multiplies,—considered merely empirically or statistically,—as we may confidently expect to be successful in disarming this cause of disease likewise of its terrors, long before the nature of the change produced by its action on the living body, or the rationale of any line of treatment of those who may be affected by it, shall become known.

The cure of Epidemic Scurvy, resulting in different cases, as is now satisfactorily established, from different deficiencies in the Diet habitually taken,—the efficacy, therefore, of different kinds of diet in counteracting this form of disease,—still more remarkably, the power of small quantities of vegetable Acids in producing the same effect, and the extraordinary rapidity with which such changes of diet, and these acids, will produce their effect,—may also be stated as examples, on a large scale, and of the most satisfactory kind, of what is generally called the power of Art over a most loathsome and virulent disease,—but in reality must be regarded, now and probably long after our time, as results obtained by simply empirical observations of the course of Nature, statistically arranged, fortunately facilitated by so many of the subjects being organised bodies of men;—and which have distinguished the present age to a degree, which those who are not familiar with the medical writings of the last century will hardly conceive.

We have good reason to hope, that inquiries now on foot as to the external causes of Scrofulous, or of what is now usually called Tubercular disease, including pulmonary Consumption,—an inquiry which we must perceive to be more complex, and in which the operation of various causes must be recognised,—will be effectual in pointing out the means of counteracting that tendency in a very large proportion of cases of persons

liable to it,—simply on the principle of empirical observation, enlarged and arranged in the form of Statistics, long before we shall have information as to the essential nature of the vital process, or mode of operation of the causes in question.

In illustration of this, I need only mention two facts, recently ascertained on so large a scale, that we have no doubt of their truth and importance, and which, even at present, may be said to be guides to successful practice in many cases only recently thought hopeless, although all that was previously known on the subject was certainly rather adverse than favorable to the supposition that they would ever be established. These are,—1. The good effect of the Cod-liver oil—if not of other animal Oils, on many cases of tubercular disease, in their early stage—provided only that it can be retained on the stomach to the extent of an ounce and a half or two ounces daily; and, 2. The almost complete exemption of the inhabitants of the Faro Islands from tubercular disease, notwithstanding that their climate, as regards cold and damp, is exactly that which, in this country, has been thought most favorable to it.—*Edinburg Med. and Surg. Journ.*

We regret that our limits will not permit us to copy the whole of Prof. Alison's interesting article. His concluding remarks are upon the operation of the New Poor Law Act.—Ed.

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*Illustrations of the Influence of Pregnancy in controlling or retarding the Development of certain Diseases.** By W. F. Montgomery, M. D. Professor of Midwifery to the King and Queen's College of Physicians.

On a former occasion, when treating of the condition of pregnancy, I took occasion to remark that it appeared from experience, that women who bear children generally enjoy more even health and are less disposed to disease than those who lead a life of celibacy, or who, having married, remained unfruitful; so that Gardien seems to express no more than the truth when he says, "Dès qu'une femme est grosse les probabilités de sa vie augmentent;" and this is what we ought, *à priori*, to expect, because, childbearing being the ordinance of an all-wise Providence, we should anticipate that the fulfilment of the duty thus ordained would conduce to the welfare of those on whom it has been devolved.

It seems in conformity with such a view to believe, what indeed, I think, experience has taught us, that pregnancy acts in a great degree as a protection against the reception of disease, and perhaps on the common principle, that during the continuance of one very active operation in the system, it is thereby rendered less liable to be invaded or acted on by another; thus it has been observed, that during epidemics of contagious diseases of different kinds, a much smaller proportion of pregnant women have been attacked than of others: but when attacked, they suffer severely; thus, when the cholera visited this country, the proportion of pregnant women who took the disease was very small, but all who caught it died, I believe, without almost a single exception. Gardien's experience led him to a similar conclusion, he says: "Les femmes enceintes sont moins exposees agagner les maladies contagieuses;

* Read before the Association of the College of Physicians of Ireland.

mais lorsqu' elles ensont atteintes, elles succombent plus promptement." I think also I have seen sufficient to satisfy me that pregnancy does, at least occasionally, exercise another kind of influence over disease in the system, namely, of preventing its development during that state, although the infection may have been caught; as is proved by the disease showing itself immediately after delivery, as in the following cases:

Mrs. W., when in the ninth month of pregnancy, was much about her brother, who was dangerously ill of malignant scarlatina; she seemed to have escaped the danger completely, but the day after her delivery she was covered with the disease, of which she died in a few days; between the time of her exposure to the infection and her delivery, there had intervened three weeks, during which she appeared to be quite well.

When Mrs. F. was in the eighth month of pregnancy, her husband had typhus fever, in which she assiduously attended him; after his recovery, she went to her father's house, some fifty miles from town, where she was delivered in due time, and immediately afterwards was seized with typhus fever, of which she died in eight days; between five and six weeks had elapsed between Mr. F.'s illness and her labor, and during that interval, she seemed in perfect health.

In the month of November, 1854, I attended a young lady in her first confinement; previous to which she had both the lower extremities much enlarged by anasarca, but she appeared, in other respects, quite well, with one exception, which was that she had such *soreness* of the abdomen, she found a difficulty in laying on either side: and when I passed my hand over the abdomen, she complained that the pressure hurt her everywhere.

On the 12th she was confined, after a favorable labor, but the abdominal tenderness remained, and there was a peculiar doughy feel of the whole abdomen; next day, this was equally felt, but with little or no pain or fever, and a perfectly quiet pulse.

On the 14th, I found the insteps of both feet, but particularly the left, covered with well-developed erysipelas; her mother, who seemed very anxious about her, was present when I examined the feet, and on our reaching the drawing-room said, "Doctor, isn't that very like erysipelas?" I said, "Yes, certainly, there was no doubt about it." "Dear me, sir, do you think she could have taken it from her husband?" She then, for the first time, informed me, that some weeks before leaving home, to come to town for her confinement, her husband had a severe attack of erysipelas, during which she had assiduously nurse-tended him. Immediately on the appearance of the erysipelas on the feet, the abdominal symptoms began to decline, and, after two or three days, ceased to exist. I cannot but believe that this lady caught the infection from her husband during her close attendance on him, that it remained in abeyance until gestation was over, and was then developed. She recovered well.

It is, I believe, a matter of common observation, that when women who have been laboring under certain forms of disease happen to conceive, the morbid affection previously existing is oftentimes either greatly mitigated, checked, or even altogether suspended for a time, as has been frequently observed in persons affected with phthisis; though I must add that the influence of pregnancy in cases of phthisis is a question on

which a variety of discordant opinions has been given by high authorities. Andral's conclusion, from his latest observations, is, "that in the great majority of cases the symptoms of phthisis are suspended, or at least remain stationary during the course of pregnancy." Louis says he is not "in a condition to determine whether pregnancy is, or is not capable of retarding the progress of phthisis," but he suggests that the fact might be, that several of the symptoms become somewhat more obscure during pregnancy, without any check being in reality given to the advance of the disease. My own experience would lead me to the conclusion, that if a woman predisposed to phthisis, but in whom the disease has not actually become developed, prove pregnant, she is likely to be benefitted thereby; and I think I have seen life thus prolonged, for years, in several instances; but, on the other hand, if pregnancy takes place in a woman already actually in consumption, or if this disease supervene on pregnancy, the fatal issue is as likely to be accelerated as postponed, or, perhaps, even more so.—*Dub'in Quarterly Jour. Med. Sei.*

Abstract of Meteorological Observations for October, 1855, *made at Philadelphia, Pa. Latitude* 39° 57′ 28″ *N., Longitude* 75° 10′ 40″ *W. from Greenwich.* By PROF. JAMES A. KIRKPATRICK.

1855. October.	Barometer. Daily Mean	Barometer. Mean Daily Range.	Thermom. Daily Mean	Thermom. Mean Daily Range	Dew Point 2 P.M.	Force of Vapor 2 P.M.	Rel. Humid. 2 P.M.	Rain.	Prevailing Winds.	Remarks.
	Inches.	Inches.	Deg.	Deg.	Deg.	Inches.	Hunds.	Inch.	Points.	
1	29.651	.205	72.2	6.2	65.5	.628	.72		NW.	M. and aft. cl'dy; ev. clear.
2	29.545	.106	68.2	4.0	61.8	.551	.75	0.326	(Var.)	Cloudy; m. and ev. *rain.*
3	29.491	.100	61.7	6.5	57.3	.470	.73	0.072	W.	M. and ev. clear; aft. cl'dy; n't *rain. Bar. l'st* 29.453.
4	29.646	.155	61.0	1.3	50.1	.362	.55		WNW.	M. cl'dy; aft. and ev. cl'r; 7 to 11 P.M. brilliant *aurora.*
5	29.761	.116	60.0	8.0	56.5	.457	.49		SW.	Clear. *Therm. highest* 77°
6	29.673	.088	64.8	5.5	58.5	.701	.92	0.683	S.	Cloudy; m. and aft. *rain.*
7	29.898	.225	51.0	13.8	36.5	.216	.43		WNW.	M. and ev. clear; aft. cl'dy; first *hoar frost* observed.
8	29.928	.045	51.8	2.7	34.9	.203	.42	0.005	WSW.	M. clear; aft. cloudy; ev. drizzling *rain,.*
9	29.934	.066	55.7	4.0	48.6	.343	.58		(Var.)	M. cl'dy, aft. and ev. clear.
10	30.046	.059	62 0	6.3	58.0	.482	.66		SW.	M. hazy, aft. and ev. clear.
11	29.851	.195	66.0	4.0	55.2	.436	.48	0.467	SE.	M. fog: aft. cl'r; ev. & n't *r'n.*
12	29.803	.080	49.7	16.3	40.9	.256	.62		NW.	M. & aft. cloudy; ev. clear.
13	29.820	.014	42.2	7.5	37.4	.224	.79	0.038	WSW.	Cl'dy; m. & night drizzling
14	29.822	.017	49.7	7.5	39.6	.243	.57		SW.	Cloudy. [*rain.*
15	29.853	.031	55.2	5.5	42.4	.271	.47		SW.	M. hazy; aft. and ev. clear.
16	29.977	.125	54.3	3.8	46.1	.312	.56		NW.	M. hazy; aft. and ev. clear.
17	30.038	.074	48.7	5.7	40.2	.249	.54		WNW.	Clear.
18	29.971	.067	54.0	5.3	50.8	.372	.63		SW.	M. hazy, aft. and ev. clear.
19	29.951	.022	59.8	5.8	60.2	.522	.79		SW.	M. fog, aft. and ev. clear.
20	29.821	.130	59.8	4.0	60.6	.530	.97	0 491	NNE.	M. fog; *rain* from 11½ A.M.
21	29.698	.123	63.0	3.2	61.0	.536	.84	0.013	(Var.)	Cloudy; m. drizzling *rain.*
22	29.731	.059	56.7	6.3	43.5	.283	.54		W.	M. hazy, aft. cl'dy, ev. clear.
23	29.810	.079	47.0	9.7	40.0	.247	.71		(Var.)	Cloudy; aft. & night *rain.*
24	29.579	.231	46.5	2.8	45.8	.308	.80	1.304	NNE.	Cl'y, *rain* continued all day
25	29.826	.247	41.7	4.8	26.9	.146	.47		W.	M. & ev. cl'r; aft. cl'y; *rain* mingled with *snow.*
26	30.001	.237	43.0	3.3	31.2	.175	.50		WSW.	M. & ev. cl'y; aft. cl'r. *Ther.*
27	29.772	.229	47.5	4.3	39 5	.243	.58	0.017	(Var.)	Cl'dy; m. *rain.* [*lowest* 34°
28	29.668	.158	50.3	2.8	32.8	.187	.42		NW.	M. & aft. cloudy; ev. clear.
29	30.052	.384	48.2	3.5	34.3	.198	.43		SW.	M. hazy; aft. and ev. clear.
30	29.927	.125	57.7	9.5	53.6	.412	.60		SW.	M. cloudy; aft. & ev. clear.
31	30.157	.196	53.3	5.0	38.0	.229	.47		NE.	Cl'dy. *Bar. highest* 30.185.
Means for Oct., 1855	29.831	.131	56.2	5.8	47.0	.348	.61	3.416	S. 88½° W. 70-100.	
Means for Oct., 6 yrs.	29.932	.133	55.7	5.4	45.9		61.	2.760	S. 73½° W. 50-100.	

The Monthly Range of the Mercury in the Barometer was 0.732 of an inch, and in the Thermometer 43°.